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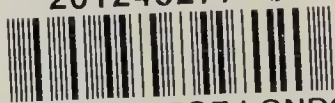
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KINGS COLLEGE LONDON







LECTURES  
ON  
SUBJECTS CONNECTED WITH  
CLINICAL MEDICINE.  
VOL. I.

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L E C T U R E S  
ON  
SUBJECTS CONNECTED WITH  
CLINICAL MEDICINE,  
COMPRISING  
DISEASES OF THE HEART.

BY  
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AND LATE PHYSICIAN TO ST. BARTHOLOMEW'S HOSPITAL.

IN TWO VOLUMES.

VOL. I.

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## PREFACE.

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SOME years ago, I published a small volume containing lectures which I had given at St. Bartholomew's Hospital, with the view of assisting the studies of those who seek the knowledge of disease at the bedside of the patient. I believe they were found useful; and in that persuasion I would have published other lectures with the same object. But an enlarged sphere of duty at the hospital and elsewhere engrossed all my time, and impaired my health, and spoiled my good intention.

The duty of physician to a great hospital, unless it can be made easy by indifference to its highest obligations, is incompatible with much care of personal health. Therefore I relinquished my office at St. Bartholomew's, and, with it, some of the best hopes I had of being useful in my generation.

But returning health brought back the recollection of interrupted purposes, and the desire of renewing them, and led me to think again of what I had written or remembered of past experience, if, perhaps, I might yet glean from it a little which would be profitable to others.

Of the lectures now published, some are literally, and some are only in substance, those which were given at St. Bartholomew's; and others have been added to them.

Their subject is, diseases of the heart. For after all that has been written upon it, something, I have thought, is still wanting to bring it within the easy reach of the medical student; and this I have endeavoured to supply.

The treatise of Dr. Hope is very comprehensive. It embraces all that concerns the heart, its physiology, its pathology, and the treatment of its diseases. But the very abundance of its matter has made it a *hard* book to the student, and its style, which is too often controversial, and even disputatious, repels many readers, and has been in some measure a hindrance to its usefulness. Yet it is a great work, and must always hold a high place in the medical literature of this country. Such information as I have to impart,

has no aim of superseding either this or any other valuable work upon the same subject, but will rather (I trust) render the student more desirous of consulting it, and more apt to consult it profitably.

Mine is a limited purpose. It is to regard the diseases of the heart only in one point of view, *i. e.* as they appear in the living man. But this one point of view includes the several objects of their clinical diagnosis, and their clinical history, and their medical treatment. These are what I seek especially to illustrate, while I presume an acquaintance with other parts of the subject, and shall only allude to them incidentally as I go along.

The other parts of the subject, indeed, include no less than all that belongs to the morbid anatomy of the heart, the productions and processes which variously alter, or injure, or destroy its organic structure. These are the very things of which clinical observation seeks to know the living signs and the living history and the treatment, both curative and palliative. But these, it is no part of clinical instruction formally to explain; yet, unless there be some previous knowledge of them, clinical experience cannot safely proceed; and, unless that

knowledge be kept up and improved concurrently with it, clinical experience can never go on to perfection.

The clinical diagnosis of diseases of the heart owes all the higher degrees of certainty to which it has been carried in our own times, entirely to auscultatory signs. Accordingly it became necessary for me to give some account of their theory and their uses, and I have desired to do it as simply as possible. Their perfect theory, however, lies deeper than our present knowledge, and all the uses of which they are capable must wait to be developed by more and more multiplied observations of the sick.

But already there is some true light in which these signs may be regarded; and already there is a large extent to which they may be followed and trusted as the faithful exponents of diseases of the heart. And although on this subject doubtless there has been error and mistake, and a good deal has been taken for more certain, and something for less certain, than it really is, so that we have both to learn and to unlearn; yet enough is already known to make the diagnosis of diseases of the heart hardly any thing else than a just appreciation of their auscultatory signs.

After I have described the auscultatory signs,



and endeavoured to show what they are, both in themselves and in relation to other symptoms, and what is their value as guides both to diagnosis and treatment, I may perhaps seem unreasonably to cut short the further description of the heart's diseases.

But as I lectured, so now I write, for one class of students especially. As my hearers were, so now I presume my readers will be, chiefly those who are seeking information at the bedside. To such there is no greater impediment of knowledge than over-teaching. The teaching which they most require is suggestive. They have the realities themselves to learn from, the original *book* to read, upon which all sound instruction is but a commentary. Therefore the commentator should only interpose when and where he is needed, and not after the manner of certain critics, who most *help* us with their annotations where the sense of the author is clear beyond dispute.

A country pastor made one of his flock a present of Bunyan's "*Pilgrim's Progress*;" and, anxious that he should both read it and profit by it, took care that the copy which he gave him should be one well furnished with notes. Meeting the man some time afterwards, he asked him

how he liked the book, and whether he was sure that he understood it; and received for answer, that he both liked it and understood it all well enough, *except the explanations.*

So with students who have free access to the wards of a great hospital, we should not be too ready in describing and commenting upon the ordinary phenomena of diseases which are constantly before their eyes, lest, perchance, they should tell us "that they understand all well enough, except the explanations."

But there is a clinical *history* of diseases of the heart, as well as a clinical diagnosis. By their clinical history, I mean the notice of those conditions, which, whether preceeding, accompanying, or following, may be deemed to hold with them some pathological kindred.

Every part, however, of such clinical history is not made out with equal clearness. That which is constructed out of subsequent events, is the most full and complete. Observation has traced back, with fearful fidelity, a long line of formidable and fatal diseases to their pathological parentage in the heart. But that which is constructed of preceding or coincident events, is hitherto less perfect; yet observation has been

able to assign to *some* diseases of the heart a sure origin in, and a still continued alliance with, diseases of other organs, or of the constitution at large.

Now it is this last part of their clinical history which is most available to practical purposes : how greatly available, may be shown from the single instance of the ascertained connection of endocarditis and pericarditis with acute rheumatism. These most formidable affections of the heart have been brought within our earlier knowledge and our earlier treatment, and so within the greater probability of cure, by our being fully aware when most to expect them, and then being upon the watch for them. And all this has come from the study of their clinical history, which has stamped their pathological kindred with acute rheumatism.

But there are diseases of the heart which have a clear clinical diagnosis, but no clear clinical history, except of events which follow them ; and these are the least amenable to medical treatment. The same signs which notify their existence, declare their incurability. What a gain will it be to mankind, should observation hereafter discover that the conversion of the valves of

the heart, and the lining of arteries, into earthy matter or cartilage, has its sure pathological origin in certain forms of disease in other parts, or in the constitution at large, which are both obvious and curable, or in certain habits and modes of living which can be rectified or avoided !

The study of our times has been chiefly to specialise and to localise disease, and it has had very useful results. But it has had a tendency to narrow our views, and to cripple our practice, by setting up as many several pathologies within the body as there are several organs. Yet no sooner do the diseases of separate parts come to be treated, than they begin to claim their place in a common pathology. We cannot reach them, and apply our remedies directly to them, in the isolated spots wherein we find them ; but if they are to be reached, and treated at all, it must be through the vascular system, or through the nervous system, or through the digestive and assimilative system. For these are the common agents of life and increase, both healthy and unhealthy, and the common channels both of food and of medicine.

Upon the treatment of diseases of the heart, and all that immediately concerns it, these lectures

may have the appearance of being unnecessarily prolix. But be it remembered, that they were originally addressed to students, from whom I could not withhold any piece of practical information which they ought to possess, and, when I could not refer them for it to other sources, I was obliged to furnish it myself. And still I consider myself addressing chiefly students, or those who are daily engaged in the obligations and responsibilities of their profession, and who desire to be faithful to them, and so cherish an habitual preference for that knowledge which is useful.

All things should have a consideration bestowed upon them in proportion to their importance: the question is, whether the treatment of diseases has, upon the whole, had as much as is due to it.

During the last quarter of a century, physicians have laboured very hard, and, upon the whole, very profitably. But their labour has been bestowed in unequal degrees, and consequently with unequal success, upon the objects which concern them. Pathology and diagnosis have had much more of their regard than treatment. Thus our knowledge of disease in its essence has been greatly enlarged, and our skill of



detecting its present existence and seat in the living body, has been made more exact and sure, while our ability of influencing its progress and events by medicine has not been proportionally increased.

But how has this happened? Is it that the mind, which is best fitted for the study of pure pathology, is naturally averse from concerning itself with practice? One would hope not; but yet it may be so. The things themselves are different, and may naturally enough please different minds. Disease is a thing of itself, and admits of being studied with little reference to other things; and this may suit one mind. The affair of treating it must necessarily suffer admixture with various accidents and circumstances of life, and cannot be conducted without constant reference to them; and this may suit another mind. But however this may be, the fact is certain, that to many eminent physicians, of foreign schools especially, to whom speculatively we owe the most, practically we owe the least. Their lessons of pathology and diagnosis are copious, original, and instructive; their lessons of treatment are brief, barren, and unprofitable.

Yet it concerns physicians, above all men, that

theirs should not be a barren knowledge, but that it should claim honour of mankind from a sense of the benefit which they receive from it. Far be it from me to contend, that every piece of pathological knowledge is to be disparaged or rejected, which cannot at once be made subservient to a practical purpose. The knowledge is to be obtained at all events, and kept ready for use, whether the use come soon, or late, or never. Use, however, is the end always to be regarded, as well philosophically as morally. An age of great increase of speculative knowledge in medicine ought, surely, to be an age distinguished by some great practical benefit.

It is much to be lamented that any eminent master of pathology, who, while he is concerned with the nature of disease, has seemed at home, and in earnest, and satisfied with his work, pleased to instruct, and gaining favour for his instruction as he goes along, should come at last to the treatment of disease as to a humbler and less worthy portion of the physician's care. For this ought not to be. Medicine, as it begins to touch upon higher interests, even the interests of life and death, should feel itself in alliance with higher motives than any which can be thought to

help and quicken its pursuit as mere science. For now it claims a sort of moral respect in the handling; it calls upon the conscience as well as the intellect, for more caution to avoid error, and more fearfulness of overstepping the truth.

The treatment of diseases, rightly considered, is, in fact, a part of their pathology. What they need, and what they can bear, the kind and strength of the remedy, and the changes which follow its application, are among the surest tests of their nature and tendency.



# CONTENTS

## OF

### THE FIRST VOLUME.

---

#### LECTURE I.

The Natural Sounds, and Impulses and Resonances of the Heart. — How their Variations of Degree and Extent become Evidences of the Heart's Disease or Unsoundness - - - - - Page 1

#### LECTURE II.

Sounds of the Heart different in kind from its natural and healthy Sounds—to be called Murmurs.—Murmurs are either Endocardial or Exocardial.—The general Characters of each.—The Endocardial considered as the Signs of a diseased Endoeardium.—Conditions to be taken into account in relation to them as such.—What Conditions have erroneously, and what have justly, been thought essential to their Meaning, as Signs diagnostic of Valvular Disease - - - - - 24

#### LECTURE III.

Endocardial Murmurs continued.—Their Origin from Valvular Disease sometimes doubtful.—May proceed from other Forms of mechanical Impediment.—Deformed Chest.—External Pressure.—Endocardial Murmurs sometimes confounded with the Murmur of Respiration.—A peculiar Murmur, akin to the Endocardial, a frequent concomitant of Pulmonary Consumption.—Endo-

cardial, arterial, and venous Murmurs, proceeding from impoverished Blood. — Exocardial Murmurs. — Their Seat the Pericardium. — Their only known Cause the Friction of its Surfaces in a State of Disease Page 54

#### LECTURE IV.

General Estimate of the Uses of Auscultation applied to the Heart - - - - - 77

#### LECTURE V.

Inflammation of the Heart.—Endocarditis and Pericarditis. — The Endocardial and Exocardial Murmurs their chief diagnostic Signs. — How they become such. — The Endocardial Murmur, though found in numerous other Diseases of the Heart, yet conditionally the sure Sign of Endocarditis. — The clinical Knowledge of Endocarditis a new Knowledge. — Its connection with Acute Rheumatism.—How it came to be distinguished, during life, from Pericarditis - - - - - 94

#### LECTURE VI.

Endocarditis continued. — Its general Description less useful than a clinical Commentary upon its Individual Symptoms.—The Endocardial Murmur, in Endocarditis, is sometimes preceded by a certain Roughness or Prolongation of the Heart's natural Sounds.—Often it arises abruptly.—Seat and Directions of the Murmur.—Its Accompaniments, Pain, Abnormal Impulses, and Actions of the Heart.—Their practical Value, as Symptoms, according to their Degree, and according to the different Relation which the Endocardial Murmur bears to them in different Cases.—Their Diversities of Relation contain Intimations respecting the Stages and Progress of the Disease.—The same confirmed by the Success and Failure of Remedies.—Their practical Importance 106

## LECTURE VII.

Pericarditis. — The Exocardial Murmur its pre-eminent Sign. — An imperfect Murmur sometimes precedes the true. — In Pericarditis, as in Pleurisy, another Auscultatory Sign beside the Murmur, Dulness to Percussion. — Their Relation to each other not exactly the same in the two Diseases. — In Pericarditis other Signs immediately referable to the Heart, besides the Auscultatory : Vibrations sensible to the Touch, Undulations to the Eye. — Other Symptoms of Pericarditis. — Their Relation to its Auscultatory Signs. — From these other Symptoms, and chiefly from its known Connection with Acute Rheumatism, Pericarditis often rightly presumed to exist, and often successfully treated ; yet often overlooked, and often treated too late, and often fatal, for want of the Auscultatory Signs - - - Page 123

## LECTURE VIII.

The frequency of Endocarditis and Pericarditis, occurring separately or together, as the Accompaniments of Acute Rheumatism. — Present Results of Endocarditis occurring alone — Of Pericarditis occurring alone — Of both occurring together. — Difficulty of gaining Knowledge of their ultimate Results, when the Cure is imperfect 143

## LECTURE IX.

Inflammation of the Lungs accompanying Acute Rheumatism, either alone, or in combination with Endocarditis, or with Pericarditis, or with both - - 159

## LECTURE X.

The Treatment of Acute Rheumatism considered, preparatory to the Treatment of its Accompaniments, Endocarditis and Pericarditis. — Acute Rheumatism successfully treated, upon different and even opposite Indications, and by different and even opposite Remedies. — How this may be without Disparagement of Medicine as a Science. — The Treatment of almost all curable Diseases narrowed to the choice of a few Indications and a few Remedies. — What the lowest and what the highest Office of the Physician. — The highest engaged in the Treatment of Acute Rheumatism and its Incidents. — The Groundwork of rational Practice is to understand the value of single Indications, and the power of single Remedies. — Treatment of Acute Rheumatism upon Indications belonging solely to the Vascular System, and solely by Bleeding. — Upon Indications belonging solely to the Nervous System, and solely by Opium. — Upon Indications belonging solely to Abdominal Viscera, and solely by Calomel and Purgatives - - Page 180

## LECTURE XI.

Treatment of Acute Rheumatism continued. — Its Treatment according to mixed Indications, and by mixed Remedies. — The Bloodvessels, the Nerves, and the Abdominal Viscera, brought simultaneously under the remedial Impressions of Bleeding, and Opium, and Purgatives. — Advantages of this Treatment. — Observations on the Use of Colchicum. — Representations of Medical Treatment often fallacious from being too favourable. — Commonly drawn from good Cases only; not from all Cases, good and bad. — The good Cases of Acute Rheumatism, or those favourable for Medical Treatment. — The bad

Cases, or those unfavourable for Medical Treatment.—  
 Notice of Cases in which Treatment succeeds or fails,  
 contrary to Expectation. — Notice of Medicines, whose  
 Operation in this Disease is unquestionably Remedial,  
 yet not understood - - - Page 210

## LECTURE XII.

Preventive Treatment of Rheumatic Endocarditis and Pericarditis considered. — In the Management of Acute Rheumatism, can any Remedy be used as specially preventive of, or any Remedy be avoided as specially conducive to, Endocarditis and Pericarditis? — Is Opium preventive? — Is Venæsection conducive? — In what their preventive Treatment really consists. — Their actual Treatment. — It should begin with the earliest Notices of their Existence. — What these are in their several Varieties. — Why, in Acute Rheumatism, the Heart, being inflamed, needs a special Treatment, while the Joints, being inflamed, need it not. — The Treatment of the Heart, however, by the same Remedies as the general Disease. — But this Treatment made special by those Remedies being used with a different Force, and in new Directions. — Bleeding and Opium used thus. — The Consideration of the Uses of Mercury postponed - 231

## LECTURE XIII.

The general Question considered of Mercury being a Remedy for Inflammation. — Conditions favouring its remedial Operation. — These found in the Nature of the Inflammation, in the Part it occupies, and in the Constitution of the Patient. — Its remedial Operation either antiphlogistic or reparatory. — The first chiefly displayed



in the Inflammations of Tropical Climates.—Parallel Forms of Inflammation hardly known in this country.—What come nearest to them.—The Effect of Mercury in these not alone, but conjointly with Bleeding.—The probable Nature of its antiphlogistic Operations inferred from its Effects, especially in two Kinds of Inflammation.—Its reparatory Operation shown by Instances

Page 260

#### LECTURE XIV.

Subject continued.—Antiphlogistic and reparatory Power of Mercury over Acute Inflammation further illustrated by its Effects in Iritis, over Chronic Inflammation by its Effects in Rheumatic Ophthalmia.—Its Effects upon internal Chronic Inflammation.—Our assurance of the Seat is generally greater than it is of the Essence of internal Chronic Diseases.—Our Conclusions, therefore, less confident respecting the Effects of Medicine upon them.—Experience of the curative Effects of Mercury in many hidden Diseases, which from Circumstances are deemed inflammatory.—Notice of a Principle to be regarded in the Mode of Administering it - - - 276

#### LECTURE XV.

Use of Mercury in the Treatment of Rheumatic Endocarditis and Pericarditis.—Every great Advance of Clinical and Pathological Knowledge requires that old Remedies should undergo the Trial of new Experiments.—In Endocarditis the remedial Power of Mercury shown, not so much by the Result of single Cases, as by the comparative Results of many which have, and of many which have not, been treated by it.—In Pericarditis its Remedial Power may be appreciated in Individual Cases.—The Relation which the Cessation of the Exocardial

Murmur bears to the Cessation of the Disease. — The power of Mercury to procure the Cessation of the Murmur, early or late, according to Circumstances. — Early Salivation most strikingly curative. — Late Salivation not without Benefit. — Comparison of some general Results

Page 294

## LECTURE XVI.

Of Endocarditis, independent of Rheumatism. — The Clinical Knowledge of Endocarditis altogether a new Knowledge. — The Way in which it was obtained suggests the Way in which it may possibly be enlarged. — Case of Acute Endocarditis ingrafted upon Chronic Valvular Disease. — Cases of Acute Endocarditis combined with Pericarditis in a previously sound Heart. — Case of suspected Endocarditis under a more chronic Form. — General Remarks - - - 321

## LECTURE XVII.

Pericarditis independent of Rheumatism. — Shown by Morbid Anatomy to be of common occurrence. — Its smaller Degrees the most frequent. — Probably harmless. — Generally beyond the reach of clinical Diagnosis. — Its greater Degrees not beyond its reach, but apt to elude it. — Why. — *Covert* Acute Pericarditis and *covert* Acute Pleurisy compared. — Review of Cases, with the purpose of finding what natural alliance Pericarditis may have with other general pathological Conditions besides Rheumatism 344





LECTURES  
ON  
SUBJECTS CONNECTED WITH  
CLINICAL MEDICINE.

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THE HEART.

LECTURE I.

THE NATURAL SOUNDS, AND IMPULSES AND RESONANCES  
OF THE HEART.—HOW THEIR VARIATIONS OF DEGREE  
AND EXTENT BECOME EVIDENCES OF THE HEART'S  
DISEASE OR UNSOUNDNESS.

OF the signs by which we judge of the healthy and morbid conditions of the heart, those that are called physical and are immediately referable to the organ itself, convey the most certain information.

“ A line drawn from the inferior margins of the third ribs across the sternum passes over the pulmonary valves a little to the left of the mesial line, and those of the aorta are behind them, but about

half an inch lower down. A vertical line, coinciding with the left margin of the sternum, has about one third of the heart, consisting of the upper portion of the right ventricle, on the right, and two thirds, composed of the lower portion of the right ventricle and the whole of the left, on the left. The apex beats between the cartilages of the fifth and sixth left ribs at a point about two inches below the nipple, and an inch on its sternal side."\*

This is the præcordial region, in which the basis and apex and lateral boundaries of the heart are denoted, and its entire outline is traced in relation to the walls of the chest.

Within this space we cannot see. But at this space we can listen, and feel, and knock, and so put it to question, whether all be right beneath. And there is no spot of it which does not in its turn make answer to the ear, to the touch, or to the tapping of the finger, and tell something of the organ that lies herein. Hence proceed sounds, some of health and some of disease, which of the two the ear must judge. Hence are conveyed impulses, some of health and some of disease, which of the two the touch must tell.

All this may seem strange at first. But it will seem no longer strange, when we consider that inseparable from the functions of the heart is a

\* Hope on Diseases of the Heart, p. 3.

certain motion or energy, and that it varies according to its conditions of health and of disease ; and that inseparable from this motion or energy are certain sounds and impulses, and that these vary as it varies. Hence these sounds and impulses have natural degrees and qualities, a natural order of succession and a natural limit of extent, which the ear and the touch can appreciate and use as a measure of the heart's health. And hence these sounds and impulses admit of deviations from their natural degrees and qualities, and their natural order and extent, which the ear and the touch can, in like manner, appreciate and use as a measure of the heart's disease.

Now it is evident that our enquiry must begin with the natural and healthy sounds and impulses of the heart. These are the standard of comparison, by which alone we can judge of the unnatural and morbid.

First, then, of its sounds. And here, for the sake of avoiding confusion, let me just mark the distinction between the sounds which reach the ear simply by listening, and those which reach it by help of percussio. Though the ear judges of both, yet are they totally different in the modes of their production. The heart itself produces the former by its own *vital* movements. We produce the latter, and the ear is made percipient of them only by our knocking. The heart contributes nothing but as an inert mass ; and what

it contributes *as such* is found equally in the dead and in the living. It is the sounds which the heart brings out of itself by its own vital movements, that I wish now to consider. The sounds, which we bring out of the heart by our percussion, I will consider hereafter: for they too carry with them notices of health and of disease, which are neither few nor unimportant.

The sounds, which naturally accompany the movements of the healthy heart, can only be learnt by the practice of listening to them. It is useless to describe them. They are simple perceptions of sense, which no words can make plainer than they are, when the ear has once become familiar with them. It is the same with all common sounds. By describing them you seek to make them known in a different way from that in which they are naturally known. Who ever thought of describing the sound of the wind or the rain except for poetical purposes? I must leave you, then, to be your own self-instructors in the healthy sounds of the heart, and recommend you to be constantly practising auscultation for the purpose on healthy subjects.

But, besides the fact, that sounds of a certain kind accompany the healthy actions of the heart, which each man must listen for and so learn for himself, there is the theory of the fact, or the explanation how these sounds arise. This surely cannot be learnt merely by listening. The fact

that it rains or it blows, we may take upon ourselves to decide without the philosophers, because we hear it. But, if we would know how it comes to do either one or the other, if we would understand the theory of winds and showers, we must enquire a little further, and betake ourselves for instruction to those who have examined into such matters.

In listening at the præcordial region, the ear at once perceives two sounds proceeding from the heart,—the one duller and more prolonged, the other clearer and shorter; the one coinciding with the systole of the ventricles and the pulsation of the arteries, the other coinciding with the diastole of the ventricles and the rest of the arteries. Hence it appears that for one pulsation of the arteries there are two sounds of the heart.

But between the two sounds of the heart there is hardly an appreciable interval. The duller sound, which goes for the first, seems to end with a snap, which goes for the second; and then succeeds an interval of repose, which is appreciable enough before the duller sound returns.

The time, thus occupied by the sounds of the heart in their succession and their pause, has been divided and accounted for after this manner:—one half is filled up by the first sound, one quarter by the second, and one quarter by the pause.

Still, though there be hardly an appreciable interval between them, the ear acknowledges two



sounds. And, that they are really two, will appear the more certain, when their efficient causes are found to be different. So much as is either known or plausibly conjectured about these causes I will now briefly state.

About the efficient cause of the first normal and natural sound of the heart there is, I am afraid, a good deal still in debate. After many direct experiments still physiologists do not agree. Pathologists and physicians then may well be pardoned any doubts and difficulties they may have about sounds which are abnormal and unnatural.

It is very plausibly conjectured that the efficient cause of the first sound is pretty equally shared between the muscular structure of the ventricles and the auriculo-ventricular valves, and that both, by the conditions under which they are placed during the systole, directly contribute to it: the muscular structure, by its contraction, giving it length and dulness, and the valves giving it a perceptible sharpness by their extension.

But what of this perceptible sharpness? Here is a new appeal to the ear. Does the ear acknowledge it? Is it indeed a *perceptible* ingredient of the first sound under ordinary circumstances? For my own part I cannot tell.

It is admitted, however, that, in the healthy heart, this sharpness of the first sound is muffled by its predominant dulness. But it is said that *there* it is notwithstanding, and that there are

states of the heart which demonstrate both its reality and how it is produced: that, in proportion as the muscular structure becomes attenuated, the first sound loses more and more of its dulness and length, and gains more and more of this sharpness, which is then not merely manifest but predominant, and sometimes exists alone; and again, that in proportion as the muscular structure becomes thickened, the first sound gains in dulness and length, and then that its sharpness is not only muffled, but absolutely absorbed and abolished.

If all this be true, the first sound of the heart is a mixed sound, or rather two sounds so blended together in the healthy organ as to be nearly or altogether indistinguishable, and so to pass for one. Disease, however, is able to analyse them for us, and, presenting them separate to the ear, makes now one and now the other predominant.

But there is yet another condition which claims a part in causing the first sound of the heart.

The muscular contraction of the ventricles already spoken of, as having its share in the efficient cause, is the same which the heart exercises in common with other muscles of the body. But there is said to be, besides, a muscular *tension*, which is peculiar to the heart. This peculiar tension takes place when the blood is poured in from the auricles, and the valves are closed, and the ventricles are filled, and their muscular fibres

braeced up, and their entire muscular mass becomes for a moment as hard as cartilage. At this moment it is that the tension reaches its *aemé* and gives a sound. And this sound of muscular *tension* mingles with the sound of the auricular valves, which are then upon the stretch, and the sounds of both mingle with the sound of muscular *contraction* which immediately follows.

If this be true, the first sound of the heart is a compound of three sounds, each having its own separate efficient cause.

This is rather an intricate piece of physiology. Yet it contains (I am persuaded) some truth. But then it looks so like a riddle, and needs so much trouble to understand and explain, that one cannot take it and use it as a ready clue to explore diseases with.

In the first sound of the heart, which is apparently *one*, there may be two or three efficient causes involved, operating *simultaneously*, and producing, in fact, two or three *simultaneous* sounds. Bearing this theory in mind I may find, perhaps, that it now and then helps me a little to explain certain auscultatory phenomena, which attend diseases of the heart. But for daily use I am content to let my ear dictate to my understanding, and to believe, that the first sound of the heart is *one* sound, and that *one* cause is engaged in producing it, viz. the muscular contraction of the ventricles.

Here, at the threshold of our subject, I would



make one general remark, which will find its application as we go along, viz. that it is not *all* physiology which can be made useful towards the knowledge and treatment of diseases, but only those parts of physiology which are undeniably true, and not only true, but easily and at once seen to be so. A great deal of what is termed physiology has turned out to be a mistake; and so far as it has got mixed up with our notions of disease (and this has happened to a deplorable extent), it has hindered the progress of practical medicine.

The efficient cause of the second sound of the heart admits of little doubt. Though it take place during the diastole of the heart, yet is it in no way produced by the alterations of form and consistence which the ventricles then undergo. Their relapse from a state of tension to a state of flaccidity has nothing to do with it; but the second sound of the heart results simply from the sudden closure of the sigmoid valves by the recoil of the blood, when it is thrown back upon them from the pulmonary artery and the aorta.

So much concerning the normal and natural sounds of the heart. As to the sounds themselves, since the ear can only become familiar with them by practice, I leave you to be your own instructors. As to their theory, taking the matters of fact and matters of speculation which have been brought to bear upon it, I consider that it is in part satis-

factorily made out, and in part only plausibly surmised. In the received theory of the second sound I am content to acquiesce; but I look to future experiments for something clearer and simpler before we can finally rest satisfied of our possessing a true theory of the first.

Of the natural and healthy *limit* of the heart's sounds within the chest something must next be said. It is a preliminary point which some have thought most important to be determined with precision. But no good ever comes from pretending to more precision than the thing itself admits of; and I am sure this matter does not admit of much. The præcordial region, it has been said, defines it. But surely the second sound always exceeds that limit, and is audible also in the course of the aorta and of the pulmonary artery and of the carotids. Indeed, nothing less could have been expected; this sound having its efficient cause in the mechanical closure of the valves which are placed at the entrance of the aorta and the pulmonary artery respectively.

With respect to the first sound, I should be at a loss to mark the exact space within which healthy proportion and healthy structure always required it to be heard, and in neither more nor less. There are so many circumstances, some consistent with health in the largest sense, and some exclusive at least of its disease, which make the systolic sound of the heart more or less exten-

sively audible, that (I am persuaded) two healthy persons would not easily be found in whom it would be heard exactly within the same thoracic space. Whether a man be fat or lean will always make a great difference. In the one it will be kept within the præcordial region, in the other it will be carried beyond it. Fat is so bad a conductor, that where it greatly abounds, it will restrict the sound to less than the entire præcordial region, even to a very small part of it; so that you will not be able to hear the heart further than you can feel its impulse, or further than its apex. But mere skin and bone are such good conductors, that in very thin persons the sound will spread very far beyond the præcordial region, and be heard at any part of the chest to which you apply your ear.

I believe that in most persons of (what is called) a nervous temperament, even when they are under no conscious excitement, the heart's sounds are to be heard beyond the præcordial region, and, under actual emotion, very far beyond it. And I believe too that in persons of this temperament the heart's sounds are apt to be of a higher intonation. One condition, no doubt, is the result of the other. In proportion as the sounds of the healthy heart are more highly intonated, they acquire a greater audible extent. The louder they are, the further you hear them. And it is the very characteristic

of a nervous heart to have its sounds both highly intonated and extensively audible.

The greater extent and louder intonation of the heart's sounds may be no direct symptoms of disease or unsoundness in the organ itself: but they may be, and often are, indirect symptoms of disease or unsoundness appertaining to other parts within the chest. Whatever gives more than their natural solidity to the contents of the chest; pulmonary deposits, inflammatory or tubercular; thickened walls of vomicae, a thickened pleura and fluid within its cavity, aneurismal tumours, and foreign growths and curvatures of the spine; all these, partly from the compression which they exercise upon the lungs from within or from without, and partly from the amount of solid matter appertaining to themselves, give to the sounds of the heart a larger audible sphere within the chest, and exalt their natural intonation.

It is well to be aware of all this. Not that the heart's sounds, by their greater audibility and extent, can do more than intimate the possible existence of some such forms of disease. The diseases themselves are discoverable by their own direct signs, and there is no need of questioning the heart concerning them. Only we should take especial care that what from extrinsic circumstances seems wrong about the sounds of the heart, be not wrongfully brought in evidence against the heart itself.

But, besides the sounds naturally annexed to the motions of the heart in its state of health, there is its impulse.

Now the fact of the heart's healthy impulse must be left entirely to the touch without further description, as that of its healthy sounds was to the ear. But there is a theory of its impulse lying beyond the immediate reach of the senses, which serves physiologists to dispute and doubt and reason about, but not to agree. How many efficient causes, or rather how many various instruments making up the efficient cause, have been spoken of as engaged in producing that single stroke which we feel between the fifth and sixth rib, I will not pretend to say. I confess that no theory of any complex agencies beyond the heart itself is verified to my apprehension. As the impulse is synchronous with the contraction of the ventricles, and the contraction of the ventricles is surely adequate to produce it, why should we not believe that it does so altogether?

The normal limit of the heart's impulse is somewhat more certain than that of its sounds; the space within which, neither more nor less, healthy structure and healthy proportion require the heart to be felt is more certain than that within which they require it to be heard. But still there are circumstances, and those not inconsistent with health, which interfere with the sensible impulse of the heart, now restricting and now extending it. The



same man, according to the varying postures of his body, will alter the place and extent of this impulse. He stands up, and makes it felt just where the apex strikes the chest, at a point between the fifth and sixth ribs, and not beyond it. He leans forward, and makes it felt both at this point and a little above it, and in the direction of the sternum. He reels upon his back, and renders it almost or altogether imperceptible anywhere. He turns on his left side, and renders it more perceptible than ever, and in a somewhat larger and different space, between the fifth and sixth ribs, and from thence more towards the mamma than the sternum. Again, he turns on his right side, and again he renders the impulse almost or altogether imperceptible.

How all this comes to pass is so obvious, that it needs no explanation. The facts themselves, however, are worth bearing in mind.

Moreover, many of the same conditions, some entirely consistent with health and some exclusive of disease in the heart at least, which make its sounds more or less extensively audible, are found capable of making its impulse more or less extensively felt: such as redundancy and want of flesh, and the proximity of consolidated structures, by a plain and appreciable operation; and such as the nervous temperament, by a more mysterious, but not less certain, influence.

But, besides the sounds and impulse of the

heart, which come from its own vital movements, and cease, when it ceases to live and to move, there are other sounds belonging to it,—sounds with which its vital movements have nothing to do, and which are (as already stated) entirely produced by our percussion of the præcordial region. These should rather be called *resonances* than sounds. They tell of solidity and hollowness. And they tell the same equally, whether the man be alive or dead; though our concern with them, as physicians, is only when he is alive.

Take the fifth costal cartilage on the left side, and let a point, midway between its junction with the sternum and its junction with the rib, be the centre of a circle two inches in diameter. This circle will as nearly as possible define the space of the præcordial region, which is naturally less resonant to percussion than the rest. In reckoning thus we suppose the frame-work of the chest fairly proportioned, no chicken-breasted sternum, no curvature of the spine, lateral or anterior, and all the organs within sound, and bearing their due relation of position to each other. Here the heart is uncovered except by the pericardium and a loose cellular texture, and may be said to lie in contact with the walls of the chest; while in the rest of the præcordial region it is covered, and separated from the walls of the chest, by the intervening lung.

In the space indicated, most practical men would (I think) be ready to admit that pereussion conveys to the ear a sense rather of less resonanee than of positive dulness. The fact is, if the pereussion used be but of moderate force you must listen attentively to make sure that the resonanee is really less here than elsewhere. It is only when the pereussion used is of a force somewhat painful to the patient, that the ear begins to acknowledge a positive dulness.

It is well to be aware, that the erect posture is more favourable than the recumbent for making this dulness or diminished resonanee perceptible to the ear; and the instant of expiration than the instant of inspiration. In the recumbent posture the intervening loose cellular texture is not strong enough still to keep the heart close to the sternum and the ribs, and prevent its receding by the force of gravity. And during inspiration either a larger portion of lung may be brought in front of the heart; or the portion which is always in front of it may be so thickened by inflation as to thrust backward, for the time, more of the heart than it actually covers.

Such are the sounds, the impulses, and the resonances, which belong to the healthy heart. Remember, the sounds and impulses are inseparable from it as a living organ, and are brought out by its own vital movements. And remember, the resonances are only conditionally annexed to



it as a passive substance, and are brought out by our percussion.

The short physiological account of them, which has just been given, will probably be found useful to us as we proceed. At all events we may make a platform of it, where we think it will bear us, and tread more cautiously upon it, where we think it will not.

Of the sounds, impulses, and resonances of the heart no other varieties have thus far been mentioned than those of degree and extent. And indeed its impulses and its resonances admit of no other varieties, either in health or in disease. But the *sounds* of the heart admit, moreover, of varieties in *kind*, which will afford abundant matter for consideration hereafter.

But let us clear up the subject as we go along, and first understand how from the sounds, the impulses, and the resonances of the unsound or diseased heart, these being just the same in kind as of the healthy heart, only more or less in degree or more or less in extent, we are able to gather such important intimations concerning the nature of its unsoundness or its disease.

A clearer sound proceeds from a thin heart and a duller sound from a thick heart; a sound of greater extent from a large heart, and a sound of less extent from a small heart. A more forcible impulse is given by a thick heart, and a feeble impulse by a thin one; the impulse is conveyed

to a longer distance from a large heart, and to a shorter distance from a small heart.

All this is surely plain enough, and it is undeniably true. Nevertheless, from its sounds *taken alone* and from its impulse *taken alone*, we could come to few trustworthy conclusions respecting the structural condition of the heart. And why? Because its sounds and its impulses are capable of being augmented or lessened, both in degree and in extent, by causes extrinsic to the heart. This has been expressly stated already : and these extrinsic causes have oftentimes a power over its sounds and impulses as great as any which the heart itself derives from diseases of its own. This will be abundantly shown hereafter.

But, happily, sounds and impulses are the interpreters of each other. The true meaning of the sound is tested by the impulse, and the true meaning of the impulse is tested by the sound.

Thus, from a clearer sound, we argue only the probability of an attenuated heart ; but we argue its certainty from a clearer sound joined with a weaker impulse. From a stronger impulse we argue only the probability of an hypertrophied heart ; but we argue its certainty from a stronger impulse joined with a diminished sound.

When impulse and sound increase together, there is probably no hypertrophy, but the heart

is only acting more forcibly from pure excess of nervous energy. When impulse and sound decrease together, there is probably no atrophy, but the heart is only acting more feebly from pure defect of nervous energy.

When the sounds and impulse of the heart are both perceived beyond the præcordial region, they give notice (generally speaking) of dilatation of one or other of the ventricles. If, under these circumstances, sound predominate over impulse, then with dilatation there is either attenuation, or somewhat less than a proportionate increase of its muscular substance. If impulse predominate over sound, with dilatation there is either hypertrophy or somewhat more than a proportionate increase of its muscular substance.

Thus it is seen how much information respecting many, the most important structural changes, which the heart is liable to undergo, may be conveyed merely by the greater or less intensity, and by the greater or less extent, of its sounds and impulses.

But, amid these sounds and impulses, what is the place and what the value of percussion? Wait a moment, and we shall see. For this matter of percussion must first be cleared a little of certain difficulties, which lie in the way of our rightly understanding its diagnostic uses in application to the heart, before we can well see what those uses are.

Call to mind the important distinctions between the sounds proceeding from the heart, which reach the ear from simple auscultation or mere listening, and those of which the ear is made perceptive by percussion. The same distinctions hold good between the sounds of auscultation and percussion, from whatever organ they proceed.

Now there is an auscultation and a percussion of the lungs as well as of the heart.

Auscultation exercises over the lungs and over the heart a peculiar and separate domain, ascribing to one and to the other what is properly its own, and marking it with a distinctive character, both in health and in disease. But percussion holds a mixed domain over both, leaving undistinguishable by any certain mark what naturally belongs to either, both in health and in disease.

The sounds of the heart, caught by mere listening, and made by its own vital movements, are *sui generis*. They cannot be mistaken for any thing else but what they are: they cannot possibly be mistaken for the sound of the lungs. And so too the sounds of the lungs, heard by mere listening, and coming from their own vital movements, are *sui generis*. They too cannot be mistaken for any thing but what they are: they cannot possibly be mistaken for the sounds of the heart.

But the sounds of which the ear is made perceptive by percussion, whether they be elicited from the heart or from the lungs, are mere degrees of resonanee or non-resonanee: they have nothing in them which is *sui generis*. Those which respect the heart may be mistaken for those which respect the lungs, and *vice versâ*. Nay! the sounds elicited by percussion from foreign substances which have their accidental growth and seat within the chest are not at all different from those belonging to the lungs and the heart.

Percussion may find the whole præcordial region dull, and much more than the præcordial region. The dull space may extend beyond it laterally, and reach from mamma to mamma; or it may extend upwards, and reach as high as the second or even the first rib on the left side, and thence, spreading beneath the sternum, reach nearly as high on the right. And all this may be caused by the heart enormously enlarged in all its dimensions. The heart, as it goes on enlarging, pushes the lungs aside, and comes itself almost in complete contact with the walls of the chest anteriorly. Still it may not be caused by the heart, but by solidified lung, or by pleural effusion, or by an aneurismal tumour, or by some vast morbid growth. By which of them, however, percussion alone cannot decide.

Thus, from the sameness of its immediate results, when it is applied to test the diseases of



organs within the chest, percussion *alone* can teach us little. And so percussion, if we begin with it, is an useless manœuvre. But percussion as an auxiliary, and in its proper place and turn, is worth a great deal. It is so especially with reference to the heart and its diseases.

We must first listen at the præcordial region with our ears, and examine it with our hands. And thus we may learn all that is capable of being known concerning the condition of the heart: the heart may disclose the whole secret of its disease by its sounds and impulses. Or thus we may only learn a small part of what is capable of being known: the heart may only give an intimation of its disease by its sounds and impulses, and we may still want the means of further information. Here percussion is often able to supply all the help that we have need of: it often comes in as an opportune expositor of the disease, and often stamps a certainty upon our diagnosis, which would be utterly unattainable without it.

Whatever forms of disease or unsoundness have the effect of augmenting the general bulk and compass of the heart, lie especially within the reach of percussion, and within the possibility of deriving illustration from it. Diseases which issue in superadded substances, in serum or lymph, or pus or blood, accumulated within the pericardium; forms of unsoundness which con-



sist of thickened muscular structure, or more capacious cavities, or of dilatation with hypertrophy, or dilatation with attenuation, all admit of being better understood by help of percussion.

## LECTURE II.

SOUNDS OF THE HEART DIFFERENT IN KIND FROM ITS NATURAL AND HEALTHY SOUNDS — TO BE CALLED MURMURS.—MURMURS ARE EITHER ENDOCARDIAL OR EXOCARDIAL.—THE GENERAL CHARACTERS OF EACH.—THE ENDOCARDIAL CONSIDERED AS THE SIGNS OF A DISEASED ENDOCARDIUM. — CONDITIONS TO BE TAKEN INTO ACCOUNT IN RELATION TO THEM AS SUCH. — WHAT CONDITIONS HAVE ERRONEOUSLY, AND WHAT HAVE JUSTLY, BEEN THOUGHT ESSENTIAL TO THEIR MEANING, AS SIGNS DIAGNOSTIC OF VALVULAR DISEASE.

OF impulses of the heart we know no other varieties than those of degree and extent: and of sounds brought out of the heart by our percussion (that is of resonances), we know no other varieties than those of degree and extent. But of sounds, brought out of the heart by its own vital movements, we know many other varieties, which are both different in kind from any that are heard in health, and different too from each other. These arise from divers conditions of disease, and so become the evidences of their existence.

This part of the subject, which respects sounds of the heart, different in kind from the natural and healthy sounds, has been rendered unneces-

sarily difficult by over-refinement. But surely this ought not to be. Any method by which we seek to make things, in their own nature confessedly perplexed and difficult, better understood, should itself be made as little perplexed and difficult as possible. If Auscultation is to be trusted for perfecting our diagnosis of diseases of the heart, auscultation must be simple.

These sounds of the heart, different in kind from the natural and healthy, have obtained many curious names from similitudes they bear to sounds of more familiar occurrence. Thus they have been called the sounds of the bellows, the saw, the rasp, the file; and the whistling and the cooing sound, and the sound of crumpled parchment, and the churning, and the rubbing, and the to and fro sound.

Now all this music or discord (call it which you will) has some reason in it, as we shall find hereafter. But we must leave it for the present, and begin with something less likely to confuse us.

First, then, for the sake of avoiding the constant recurrence of the same epithets, let us agree to designate all sounds of the heart, which are unnatural in kind, by the name of Murmurs. And, inasmuch as they are always produced by conditions found either within the cavities, or upon the external surface, of the heart, either inside or outside of it, let these murmurs be called Endocardial or Exocardial.

The endocardial murmur is not only different in kind from the natural sounds of the heart, but it takes their place, and is heard in their stead. It comes exactly where the first sound, or where the second, or where both sounds should be. It keeps strict time with the systole or with the diastole of the heart, or with both.

The exocardial murmur, too, is different in kind from the natural sounds of the heart. But it does not take the place of them. It is not heard in their stead. In proportion as it is louder, it obscures or overpowers the natural sounds. But the natural sounds are still apt to reach the ear through the exocardial murmur; and, when they do not reach the ear, it is because they are imperceptible under the circumstances, not because they cease to exist.

It would be time and trouble thrown away to dwell long upon these endocardial and exocardial murmurs, with a view of describing what they are in themselves and in contrast with each other. For after all every man must learn them for himself by the teaching of his own ear. Touching, however, our mere perception of them as sounds, there are a few circumstances interesting enough to mention, which may chance to help the ear to a readier acquaintance with them.

Whenever we hear any unusual sound, either for the sake of conveying our notion of what it is to another, or often for the sake of being sure

that we have a right notion of it ourselves, we are apt to set about imitating it. Now, any man hearing the endocardial murmur for the first time, as it occurs in the great majority of cases, would be almost sure to try and imitate it with *his mouth*, and, what with whistling and blowing, he would presently hit upon something so very like it, as to make him pleased with his own cleverness. But, hearing the exocardial murmur, such as it is in the majority of cases, for the first time, he would never think of imitating it with his mouth; he would rub his hands together or the cuffs of his coat, or take up any two things within his reach—two pieces of thick paper, perhaps—and rub them together, and, what with brushing, and rustling, and crumpling, he would presently bring out a very near counterfeit of the exocardial murmur.

But these murmurs are to be caught quickly, and distinguished surely, and turned to a ready use, only by practice. Yet it gives a previous confidence in the reality of a distinction between them, to know that the endocardial murmur conveys to all ears the idea of *blowing*, and the exocardial murmur the idea of two bodies moving in contact with each other.

It may be further stated among their general characteristics, that the endocardial murmur is most frequently a single sound, being coincident either with the systole or diastole of the heart;



yet that sometimes it is a double sound, being coincident with both: but that the exocardial murmur is rarely less than a double sound. Moreover, that the endocardial murmur is commonly more inward and deeper, and further from the ear, and the exocardial murmur more outward, and nearer to the surface, and eloser to the ear.

And now for the modes in which these murmurs are severally produced, or their efficient causes.

First, then, as to the endocardial murmur: it results principally, and most frequently, from unusual vibrations communicated to the parties of the blood by obstacles, which it encounters in its passage through the heart. The obstacles which thus set the whole conflict a-going are inherent in certain portions of the endocardial membrane rendered unsound by disease. It is possible, indeed, for the blood, spontaneously, or at least independent of any known obstacle which it encounters, to allow vibrations among its parties from which the true endocardial murmur may arise. Cases showing the fact are not of unfrequent occurrence. They form a class of themselves; and a very interesting class it is, and deserving our separate consideration. At present, however, we will limit our attention to the endocardial murmur, which has its origin in an unsound portion of the endocardial membrane.

The membrane which lines the cavities of the heart is very liable to disease, but not equally so



in every part. Where it is thin and transparent, and admits the colour and character of the muscular structure upon which it is spread to be seen through it, it is seldom found diseased; but where it is of a denser structure, either in itself or from an admixture of other structures, whether cellular or fibrous, with its own, it is frequently and often exclusively diseased. This character of a denser texture belongs to it where it forms the tough white circles which surround the apertures of communication between the auricles and ventricles: also where it is reflected upon itself, and forms the loose duplicatures of membrane which are given off, as it were, from the internal surface of the heart, either at the fibrous circles intermediate between the auricles and ventricles constituting the tricuspid and the mitral valves, or at the commencement of the pulmonary artery and the aorta, constituting the semilunar valves.

It is remarkable how curiously disease is apt to limit itself to the spaces just pointed out. Of the fibrous circle between the auricle and ventricle, of the valves which originate from it, and of the tendinous cords which connect the valves with the carnæ columnæ, there will not perhaps be the smallest space free from disease; but the disease will abruptly stop where the tendinous cords cease and the carnæ columnæ begin. The membrane, however, where it covers the fleshy columns of the heart, is not exempt from the

possibility of disease; but, when disease actually affects it, it has seldom originated there, but has generally spread from other parts of the same membrane, although (as we have just remarked) it is apt to stop short before it reaches it. •

Observe, I here speak of *disease* in the loose sense in which it is commonly taken, the sense, in which a very small part is made to stand for the whole. I mean only its local products and results, not its vital actions and processes, which mainly constitute its essence.

Any material product, then, of disease of the endocardium, a pearl of lymph adhering to it, a loose excrescence hanging from it, any interstitial thickening, any deposit of earthy matter or cartilage, is enough to produce an eddy of the blood, and so give occasion to the endocardial murmur. Hence, wherever this murmur exists we are at once led to think of an unsound endocardium; but we cannot at once be sure that it is actually unsound. For the exceptional cases, in which the murmur is coincident with a sound endocardium, are by no means rare. As soon, however, as upon reflection we have reason to believe the endocardium actually unsound, we may be almost sure that it is unsound in some portion constituting a valve. For the exceptional cases are indeed extraordinarily rare, in which unsoundness is found affecting other parts of the endocardium and leaving the valves intact.

Of all parts of auscultation there is none more

interesting than the diagnosis of valvular disease by means of endocardial murmurs. The history of its progress to its present degree of completeness is itself most interesting and instructive. I will give a short sketch of it.

And, first, I would remark generally, that, with respect to the physical signs especially which denote disease of the endocardium and its valvular structure, as well as disease of the pericardium, the student, who starts from the level of our present knowledge, has a vast advantage over those who are his predecessors by a few years. He can be at once put in the way of so surely convincing himself of certain truths, that, by a little careful observation, he will soon make them parts of his own knowledge and experience; whereas we were obliged to work them out for ourselves.

Laennec has indeed been in our hands for nearly these thirty years. And Laennec was the great originator of the auscultatory method of diagnosis in its application to the heart more strictly and especially than in its application to the lungs; and without him to show us the way, this rich and ample field of enquiry might never have been known or cultivated at all. But Laennec, in working out his proofs in detail, admitted some capital errors, which had well nigh made shipwreck of the whole discovery for any useful or practical purpose. Such was that error

of his which ascribed (what is called) the second sound of the heart to the contraction of the auricles.

Now the mischief did not so much consist in making the auricles contract with a sound, as in taking the sound thus produced *for a notice of time*, to which he referred certain unnatural murmurs, and fixed their seat and determined their import accordingly. As thus; all murmurs coincident with, or immediately consecutive to, this contraction of the auricles, *i. e.* taking place of the second sound of the heart, he held to denote disease of the valves which immediately succeed the auricles in the course of the circulation, viz. the tricuspid or the mitral.

You may conceive what errors of diagnosis must have followed the general acceptance of this erroneous matter of fact!

The proof, by experiment, that the auricles have nothing to do with the second sound of the heart, or with any sound at all, was the first great step towards a safer and surer appreciation of the diagnostic value which belonged to endocardial murmurs.

Still we did not get on. It was easy to affirm, from the presence of endocardial murmurs, that valvular disease existed. And the more prudent and more experienced learnt to be content with affirming thus much and no more; while the less wary, who ventured to commit themselves to a

diagnosis of the particular valve which the disease occupied, were very often wrong.

Still (I say) we did not get on. Still we were striving in vain to reach a more accurate knowledge of valvular disease by means of endocardial murmurs. And the main obstacle to our success turns out to have been another error of Laennec, consisting in a certain fallacious canon which he laid down, and which was for a long time generally accepted upon the warrant of his authority.

The fallacious canon was this, that each cavity of the heart was instrumental, by its contraction, in producing the murmur which proceeded from the injured valve immediately beyond itself. Thus, when the aortic valve was injured, the systole of the left ventricle produced the murmur by forcing the blood through the narrow aortic orifice. When the mitral valve was diseased, the systole of the left auricle produced the murmur by forcing the blood through the narrow auriculo-ventricular orifice. And, since the first natural sound of the heart came from contraction of the ventricle, and the second sound (as was thought) came from the contraction of the auricle, it was only necessary to ascertain in the place of which of the two sounds the murmur came, that you might be sure which of the two valves was diseased. If in the place of the first, it was the aortic; if in the place of the second, it was the mitral.



But by maturer observation it was found that this canon would not hold, and that mere coincidence of the murmur with the point of time, belonging to the first or second natural sound of the heart, would not determine which valve was diseased. When the murmur was in the place of the first sound the disease indeed often turned out to be, where Laennec would have it, in the *aortic valve*; but just as often did it turn out to be, where Laennec would not have it, in the *mitral valve*. And, when the murmur held the place of the second sound, the disease was always in the aortic valve, where it should never be, and never in the mitral, where it should always be, according to the canon of Laennec. Thus the right key was hitherto plainly wanting to the interpretation of the whole matter.

At length there was good reason to believe that the right key was furnished to us by the doctrine of regurgitation; in other words, by the general fact that, under certain conditions of valvular disease, the blood is not only impeded in its course onwards, but that it docs, and must in part flow backwards.

Only consider for a moment the proper office of the valves. They are meant (as it were) to keep guard at the orifices of the heart, and throw them wide open to the onward course of the blood, and hold them close-barred against its reflux current. But disease spoils their fitness sometimes for this office, and sometimes for that.

In one case it thickens their texture, and hurts their plianey, so that they cannot fall back and clear the way as completely as they ought, but must leave cheeks and hindrances when the passage should be entirely free. In another case it shortens and puckers them, and alters their shape, so that they never shut their orifices as they ought, but leave a chink or an aperture when the passage should be entirely closed.

Thus whether the blood be forced onwards or backwards through a narrow passage, a murmur will equally result. The auscultatory sign will be wanting in neither case.

The following are among the prominent facts which suggested, and taught, and established this doctrine of regurgitating murmurs. And it must be owned, that they are sufficiently puzzling and inapplicable upon any other theory.

In numerous well-watched cases, where a single murmur, constantly and uniformly coincident with *the systole* of the heart, had been heard during life, the valve at the entrance of the aorta, and this valve only, was found diseased after death. Here the murmur marked the time of the blood passing *onwards* from the ventricle into the aorta, through an orifice only half open, which should be open altogether. This was no murmur of regurgitation.

Again, in numerous cases, where a single murmur had been heard during life, but constantly



and uniformly coincident with the *diastole* and not with the *systole* of the heart, still this same valve at the entrance of the aorta, and this valve only, was found diseased after death. Here the murmur marked the time of the blood recoiling *backwards* from the aorta towards the ventricle, and partially re-entering it through an orifice only half-closed, which should be closed altogether. This was the genuine murmur of regurgitation.

Again, in numerous cases, where two murmurs had been heard during life, one coincident with the *systole*, the other with the *diastole* of the heart, still this valve at the entrance of the aorta, and this valve only, was found diseased after death. Of these two murmurs, proceeding from one and the same orifice, the latter was the genuine murmur of regurgitation, and not the former.

It appeared, then, that the aortic valve, in its states of disease, was capable of becoming the seat of two murmurs, one regurgitating and the other not; of either separately in different cases, or of both together in one and the same case.

Yet again, in numerous cases, where a single murmur, constantly and uniformly coincident with the *systole* of the heart, had been heard during life, the mitral valve, and it alone, was found diseased after death, while the aortic valve was perfectly healthy.

But how could this be explained? In its natural course it is during the diastole that the blood passes through the orifice guarded by the mitral valve from the auricle into the ventricle. Here, however, the mitral valve being diseased, the murmur does not mark the time of the blood passing *into* the ventricle by the mitral orifice, but the time of its passing *from* the ventricle by the aortic orifice: yet there was no disease of the aortic valve to cause it. The only material thing, capable of producing it, was still the diseased mitral valve. But how could this produce it? Even by admitting the regurgitation of blood back into the auricle. And the very point of time, at which the murmur takes place, marks this for the cause, and this for the manner of its production.

The same systole of the ventricle which carries the blood forwards into the aorta, without impediment and without a murmur, where there is no disease, throws it back partially, and with a murmur, into the auricle, through the half-closed mitral orifice, which now admits its regurgitation.

It has been said that the aortic orifice of the heart may be the seat of two murmurs, in consequence of disease of its valve; one systolic, from the blood in its direct course, the other diastolic, from the blood during regurgitation. Either murmur may occur alone in different cases, or both may occur together in the same case. But it would

almost seem that the mitral orifice could be the seat of only one murmur, and that murmur the systolic.\* Remember, the systolic murmur proceeding from the mitral valve always implies regurgitation.

Yet the condition of disease in the mitral valve is often found to be such as must have raised certain impediment to the passage of blood from the auricle into the ventricle. Why, then, is the murmur, which would indicate such impediment, and which would be coincident with the diastole of the heart, a thing not found in practice, when the mitral valve alone is diseased?

It is probable that, as in health, when the mitral orifice is entirely free, the blood glides from the auricle into the ventricle without any impelling force from behind; so in disease, when the orifice is narrowed, the resistance does not produce any extraordinary effort on the part of the auricle to overcome it. And thus in disease, as well as in health, through a narrow passage as well as a free one, the onward current of blood from auricle to ventricle is still without noise. That it is otherwise with the regurgitating current through the same passage, and that the murmur of the blood rushing backward from ventricle to auricle should be often signally loud, must be

\* The cases are so rare, in which either the diastolic murmur alone, or the systolic and the diastolic murmurs together, can be fairly imputed to the mitral valve, that they are a sort of clinical curiosity.

owing to the force of the ventricle now engaged in impelling it.

Thus by listening to endocardial murmurs during life, and noting the exact time at which they occur, whether synchronously with the systole of the heart, or with its diastole, or with both, and then by ascertaining the exact seat of disease within the heart after death, physicians had arrived at a just explanation of the way in which those murmurs are produced. They saw such a mechanism, formed by disease of the several valves, as being played upon by the blood during the vital movements of the heart, must needs have given occasion to the murmurs which they heard; to the direct murmur in one case, to the regurgitating murmur in another, and to both of them in a third.

Now this doctrine of valvular regurgitation, which brought with it a clearer insight into the whole rationale of endocardial murmurs, was pathologically a great step in advance. But in diagnosis it rather seemed a step backward; for it undid much of our former knowledge by convicting it of error. We were constrained to give up much which we once believed from seeing that it could not possibly be true. Our better pathology was for the present sorely puzzling to our diagnosis.

When we heard a loud endocardial murmur during life always accompanying the systole of the

heart, and when we often found after death the aortic valve diseased and the mitral sound, and just as often the mitral valve diseased and the aortic sound, our pathological notions were equally satisfied in either case. In the former we acknowledged the sufficient cause of the *onward* murmur, in the latter the sufficient cause of the *backward* murmur, and we acknowledged the systole of the ventricle equally instrumental in producing both. But still we could not tell which was which *during life*, or when the aortic, or when the mitral valve was unsound. The same endocardial murmur, at the same article of time in all cases, could not inform us which it was in any.

But at length we seem to have made a nearer approach to unravel these difficulties of diagnosis, and to discriminate the exact seat of endocardial murmurs during life, as well as understand the mechanism of their production. For this purpose, however, there are more things to be taken into account respecting them than their mere coincidence in point of time with the systole or diastole of the heart.

Besides this coincidence, two general facts have been put forward, as surer interpreters of endocardial murmurs, in giving them a more exact meaning, and assigning them to the particular orifice from which they proceed. The first fact is, that endocardial murmurs are most plainly audible at that part of the præcordial region which



is nearest to the orifice from which they proceed. The second fact is, that endocardial murmurs are conveyed sometimes in one direction and sometimes in another, and that the orifice from which they proceed determines in each particular case what that direction shall be.

Of these two general facts, I am more sure of the second than of the first, and have better proof of its practical use. But we will briefly consider them both.

“A line drawn from the inferior margins of the third ribs across the sternum passes through the pulmonic valves a little to the left of the mesial line, and those of the aorta lie behind them, but about half an inch lower down.”\*

“A horizontal line drawn through (along?) the under edge of the sterno-costal articulations of the fourth ribs will cut across nearly the middle of the length of the mitral valve, when drawn outwards and downwards by its tendinous chords and columnæ carneæ, and pass about two or three lines above that portion of the tricuspid which most nearly approaches it, the latter valve lying underneath the sternum, and the former immediately to its left.”†

So much of the sternum as these lines include to the left of the mesial line, and the space they indicate between the lower margin of the third

\* Hope, on Diseases of the Heart, p. 3.

† Joy, in Library of Medicine, vol. iii. 258. in a note.



and the lower margin of the fourth sterno-costal cartilages on the left side, may be taken to mark that portion of the præcordial region, behind which lie all the orifices of the heart and a good share of the valvular structures appertaining to them. Now, inasmuch as the several orifices are found at the basis of their respective valves, the pulmonary and aortic orifices must be lower than the first horizontal line, and the tricuspid and mitral orifices must be higher than the second. How nearly, then, must they all approach one another in the mid-space between them both! So nearly, that the mouth of an ordinary-sized stethoscope would surely cover them all within the circle of an inch and half or less. Whichever orifice of the heart be affected, we are sure to find the endocardial murmur here or hereabout. And listening here and here only, we cannot segregate the murmur of one orifice from that of another. What then, if "endocardial murmurs *are* most plainly audible in that part of the præcordial region which is nearest to the orifice from which they proceed!" This general fact, taken alone, cannot help us much in determining which of them is affected in a particular case, when they all lie clustered together at the same, or nearly at the same, part of the præcordial region.

But suppose we raise our ear, or the stethoscope, from this exact spot, and shift it an inch or two higher or an inch or two lower. Higher

we may hear the endocardial murmur still, and lower we may lose it altogether. Or higher we may lose it altogether, and lower we may hear it still. Or both higher and lower we may still distinctly hear it. By this procedure we are following the endocardial murmur in the direction it takes after it leaves the orifice from which it is propagated; and we find how various the direction is, upwards in one case, downwards in another, and both upwards and downwards in a third. But still it is the orifice, from which it is propagated, that gives the murmur its particular direction; and this (it is said) may be taken for a general fact.

Accordingly, when the endocardial murmur is conveyed in an upward direction, even above the basis of the heart, and still along the course of the aorta, and further still, as sometimes happens, along the subclavian and carotid arteries, the aortic orifice is its point of departure, and the valve, there situated, is the valve diseased. When it is conveyed in a downward direction, and to the apex of the heart, the auriculo-ventricular orifice is its point of departure, and the valve, there situated, is the valve diseased. And when it is conveyed both in an upward and downward direction, both in the course of the aorta, and to the apex of the heart, then it has two points of departure, and both the aortic and the mitral valves are diseased. Here the murmur, which is

*one* to the ear, may be *two* in fact. The two are made one by being both synchronous with the systole of the ventricle. In this case the murmur from the aortic orifice is direct, and that from the mitral is regurgitating. Or the murmurs thus conveyed in different directions, as they are two in fact, may be two to the ear. But then one must be synchronous with the diastole, the other with the systole of the heart. In that case the diastolic murmur comes almost always from the aortic and the systolic from the mitral orifice; and the diastolic and aortic murmur is not direct but regurgitating, and the systolic and mitral murmur is regurgitating still.

The general fact, that endocardial murmurs pass in certain directions according to the seat of the valvular disease, has yet a further interest and use. It helps us some way towards the differential diagnosis of diseases of the same order of valves on the two sides of the heart.

When the murmur, audible in the space between the two horizontal lines above described, is conveyed upwards and beyond the basis of the heart, the disease may be either of the aortic or of the pulmonic valves. The direction that it takes from this point must determine which of the two; for it may take more than one direction. When it is heard passing upwards for the space of two inches, and between the second and third ribs of the right side, then it is taking the course

of the aorta, and the disease is of the aortic valve ; and still more surely, if it be heard in the carotid arteries. But when it is heard passing upwards between the second and third ribs, not of the right but of the left side, then it is taking the course of the pulmonary artery, and the disease is of the pulmonic valve ; and still more surely, if it be not at all heard at the same time in the carotids.

I doubt whether it would be possible to arrive at a differential diagnosis of diseases of the mitral and tricuspid valves from any ascertainable difference in the direction to which the murmur originating from one or the other is conveyed.

It should be remarked, that valvular disease on the right side of the heart alone is a most rare occurrence ; and that, when it is found on both sides together, the disease on the left generally so far outruns that on the right, as to have reached its acmé before the other has hardly begun. Hence, in the vast majority of cases, valvular murmurs proceed from the left side exclusively ; and, in the few cases, where they proceed from both, those from the left will probably be so much the loudest as to overpower those of the right. And in the still fewer cases, where they proceed exclusively from the right, they are submitted too rarely to our observation for us to be familiar with the peculiarities which belong to them. In truth, almost all our knowledge of endocardial murmurs, proceeding from valvular disease, is derived from our



study of those which appertain to the left side of the heart.

Finally, then, is the doctrine of valvular regurgitation, are the notices of time marked by the systole and diastole of the heart, is the fact of sounds being heard more plainly in one part of the præcordial region than another, or the fact of sounds being conveyed in this or that direction within or beyond the præcordial region, forward or backward, with the current of the blood; are all these facts true and stable, and general enough to hold the place of principles? And, if they be, will they bear to be taken, always and without reserve, as the sure exponents of endocardial murmurs, so far as to fix the valve or orifice of the heart from which they proceed? I dare not affirm so much. I do, indeed, still make use of them as principles, but I am less peremptory about the certainty of their application than I was a year or two ago. Often the event has been just as they would indicate. But occasionally it has been contrariwise; and the exceptions I have not always been able to explain without prejudice to the assumed principles.

But we should not be in a hurry to abandon such general facts as these which have often led us right, because they have sometimes seemed to lead us wrong. We should rather suspect the occasional interference of counteracting circumstances, which we do not yet understand. In the

history of our profession we meet too often with things utterly worthless capriciously taken up, but sometimes with things really valuable capriciously laid aside.

Among the conditions, which may possibly intervene to turn endocardial murmurs from the direction in which the disease of particular valves would tend to convey them, the following may be mentioned:—

1st. The presence within the chest, and exterior to the heart, of substances having a more solid consistence than its natural contents, such as morbid growths of various kinds, or aneurismal tumours, or condensed portions of lung. These are able to conduct the abnormal murmurs, no less than the natural sounds, of the heart, to a greater distance, and in any direction, according to the place they occupy.

2dly. The enlarged capacity of the heart itself, which is the most frequent consequence and concomitant of its diseased valves. The large dilated heart spreads its sounds abroad laterally. And thus, whether the murmur be traced in the course of the aorta, or not at all above the basis of the heart, it is often as loudly audible from mamma to mamma, and every where in front of the chest below the fourth ribs, as in the præcordial region itself; and often even far round towards the left axilla.

3dly. The mere loudness of the endocardial



murmur. The abnormal murmurs, as well as the natural sounds, of the heart, are heard to a greater distance in proportion to their mere loudness, and that not only in the directions to which the current of the blood conduets them, but in all directions.

Now, when these three conditions meet; the loud endocardial murmur, itself very widely audible, and the enlarged heart, ready to spread it still further abroad, and some solid substance within the chest ready, according to what its seat may be, to conduct it in any new direction, no wonder that the tendency of a diseased valve to convey and to restrict the same murmur within a particular channel should be sometimes counteracted and disturbed.

To these several conditions I may add a fourth, viz. a peculiar quality of the endocardial murmur, giving it a high musical note. Such a murmur will sometimes refuse to suffer restriction to any certain space within the body. It will even carry itself outwards and reach the ears of bystanders at a short distance.

Touehing endocardial murmurs, as the signs of endocardial disease, there remain two more points to be considered. They have been relied upon, not only for fixing its seat in this or that orifice of the heart, but also for estimating its magnitude and the amount of impediment raised by it to the passage of blood, and for determining

*the kind* of structural change which it has produced.

The popular notion seems to be, that the louder the murmur the *greater* the disease, and the greater the amount of impediment.

The truth, however, is, that the murmur becomes louder as the disease and the impediment increase only *up to a certain point*, and then, that it becomes less and less loud as they go on to increase beyond this point. Thus the disease and the impediment still increasing may, and sometimes do, reach a point at which the endocardial murmur ceases thenceforth, and altogether, as long as life remains.

Two individuals of unsound heart died within a few days of each other. I witnessed the symptoms of their disease during life, and after death I saw what that disease actually was. In both the right ventricle was dilated and the left was dilated and hypertrophied; and in both the mitral valve and the aortic valve were diseased. But the valvular disease, and the impediment resulting from it, were far greater in one case than in the other. In the one the auriculo-ventricular orifice was so narrowed as only just to admit the little finger, and the aortic orifice was only just not closed. In the other there remained a tolerably free space for the passage of blood through both orifices.

Now in the first case during life there was

no endocardial murmur at all; while during life in the second there was a loud bellows-murmur audible in the whole præcordial region, and far on either side of it, and beyond it upwards in the course of the aorta.

All this seems to admit of easy explanation. When endocardial murmurs result from diseased valves, there are two agents engaged in producing them, viz. the mechanical obstacle which the blood encounters, and the blood itself. It is from unusual vibrations among the particles of the blood that the unusual sound immediately proceeds; but it is the obstacle which sets the conflict agoing. Now the sound must be in proportion to the vibration; and the vibration is in proportion to the amount of the obstacle and the quantity of blood and the rate at which it circulates taken together. Thus the endocardial murmur becomes louder and louder while the valvular disease is upon the increase, as long as the heart by its increasing thickness is still able to force a large current of blood through a moderately contracted orifice. But the endocardial murmur becomes fainter and fainter, and at length ceases altogether, as the valvular disease, by its further increase, goes on still to narrow the orifice, and the ventricle with all its increasing thickness can only force the blood through it in a more and more slender stream.

Further, there is, or, rather, perhaps there was,

a notion that endocardial murmurs have wonderful diagnostic secrets wrapped up in their varieties of *kind* and *quality*; and that all those similitudes, which they are in different cases found to bear to the sounds of the bellows, the saw, the rasp, the file, or to whistling or cooing, were worth our serious study, inasmuch as they severally denote the very *kind* of structural change which a diseased valve has undergone, whether it be converted into cartilage or earthy matter, into matter hard or soft, or rough or smooth. Experience, however, does not countenance the belief, that the *kind* of endocardial murmur follows the *kind* of endocardial disease.

But upon what do the varieties of murmur which accompany valvular disease really depend? Go into the wards of this hospital, where there are always numerous cases of diseased heart ready for observation, and perhaps you will find three or four or half a dozen patients, in whom the endocardial murmur is strongly marked, and has those accompanying conditions (you know what they are) which make it highly probable, not only that the disease is valvular, but valvular disease of the same orifice in them all. But the murmur will have as many varieties as there are patients; yet it will be characteristically endocardial in all, while it is different in each.

Then go into the museum, and scrutinise half a dozen specimens of disease in a particular valve.



Let it be the same valve which you believe to be the seat of disease in the patients whom you have been examining in the wards. In all the specimens you will find the orifice to which the valve belongs obviously narrowed, but so narrowed as to leave the stricture of different size and different form in each. Thus the orifice will have become a pipe or a funnel or a chink in the several cases. In one it is direct, in another tortuous; in another it has a bar drawn across it, and has two apertures; in another it has several bars drawn across it, and is cribriform.

No wonder then will any longer remain, that in the wards of the hospital the patients whom you believe to have the same valve of the heart diseased should all present the characteristic endocardial murmur, but each a different variety of it. The murmur, with its accompanying conditions, denotes the valvular impediment and the orifice it occupies, while its varieties arise from the different sizes and shapes of the orifice through which the blood has to pass, and the rate at which it passes.

Upon the whole, my persuasion is, that no practical good has come from curiously naming, and noting, and multiplying endocardial murmurs. The *mere* murmur can only tell me whether it proceed from the inside or from the outside of the heart. For more than this I cannot trust it. But in telling me this, it tells that which I have no possible means of knowing without it.

Having determined that the murmur is endocardial, and proceeds from within the heart, if I desire to know, moreover, whether it arise from valvular disease, and from valvular disease on which side of the heart and at which orifice; then for this more exact diagnosis I must add to the mere endocardial murmur a reckoning of the time at which it occurs, and a reckoning, too, of the space within the præcordial region at which it is chiefly heard, and of the direction in which it is conveyed.

And if (what is most important of all) I aim at a diagnosis of the endocardial disease in respect of its essence and nature, then to the mere sound, and its time and its place and its direction, I must *add* a reckoning of the actions and sufferings of the constitution at large which precede it and attend upon it. These, which are the highest considerations of all, are reserved for their proper place.

In the mean time I would observe of the mere murmur, that nothing would be lost in propriety of language, and much gained in simplicity, if the term *endocardial* were made to include all its ordinary varieties which proceed from within the heart, and were the single term in common use; and if the fantastic similitudes which have been mentioned were only now and then employed to help us in describing something extraordinary, it would be all the better.



## LECTURE III.

ENDOCARDIAL MURMURS CONTINUED. — THEIR ORIGIN FROM VALVULAR DISEASE SOMETIMES DOUBTFUL. — MAY PROCEED FROM OTHER FORMS OF MECHANICAL IMPEDIMENT. — DEFORMED CHEST. — EXTERNAL PRESSURE. — ENDOCARDIAL MURMURS SOMETIMES CONFOUNDED WITH THE MURMUR OF RESPIRATION. — A PECULIAR MURMUR, AKIN TO THE ENDOCARDIAL, A FREQUENT CONCOMITANT OF PULMONARY CONSUMPTION. — ENDOCARDIAL, ARTERIAL, AND VENOUS MURMURS, PROCEEDING FROM IMPOVERISHED BLOOD. — EXOCARDIAL MURMURS. — THEIR SEAT THE PERICARDIUM. — THEIR ONLY KNOWN CAUSE THE FRICTION OF ITS SURFACES IN A STATE OF DISEASE.

IF ever a single sign could be taken at all times to denote one thing and one thing only, you might think perhaps it must be the endocardial murmur to denote mechanical impediment from valvular disease. The theory of its production by this cause is very plain and intelligible; and many a man's experience may have run uniformly in confirmation of it in every instance, without exception, which has fallen under his notice. But still individual experience, be it ever so large, is not all experience; and truths without exception are not the truths most commonly met with in medicine.

There are cases of endocardial murmur in which valvular disease is at least doubtful; and cases of endocardial murmur in which there is mechanical impediment but no valvular disease; and cases in which there is neither mechanical impediment nor valvular disease; and there are cases, too, in which the ear itself is apt to be deceived into the belief of a murmur proceeding from the heart, when there is no such murmur in fact.

Now it is unwise so to treat of any medical subject as if it were complete. Yet nothing, it must be allowed, is more useful than to give that order and arrangement to the many *accordant* facts of medicine into which they naturally fall. But still a place must be found for other facts, few in number, which are really or apparently contradictory to the rest, or at least do not altogether harmonise with them.

Here, then, I must find a place for some of the rare facts just alluded to, which have fallen in my way, touching endocardial murmurs. I will state them, and comment briefly upon their pathological bearings and relations as I go along.

I occasionally find the endocardial murmur under circumstances unlike those usually attending it when it is the undoubted result of valvular injury, and yet it probably *does* proceed from valvular injury nevertheless.

There are cases in which the murmur is not constantly present. It comes and goes; and the

circumstances under which it comes and goes are to interpret for us the nature of the disease out of which it springs.

This has sometimes occurred to my observation. The patient has been sensible of uneasiness in the region of the heart, and of occasional palpitation. Upon examination I have found the impulse slightly in excess, and the sounds louder and more diffused than natural, and nothing more. But these are the common accompaniments of nervous disorder; and accordingly I have been about to conclude the heart to be perfectly sound, and to dismiss my patient with the comfortable assurance that such was my belief. For my further satisfaction, however, I have made him walk briskly once or twice round the room, and then listening again, I have found the impulse of the heart considerably augmented, and an unquestionable murmur. A few minutes of quiet have moderated the impulse, and stilled the murmur: but he has taken another turn round the room, and both impulse and murmur have returned. And thus has the murmur been audible on exertion, and inaudible on repose, several times in a quarter of an hour.

Now here, where we can make the murmur come and go at will, simply by augmenting and reducing the force of the heart's action, it is reasonable to believe, that there may be a mechanical obstacle at an orifice of the heart, but that

it is of small amount; not enough to cause the requisite degree of vibration when the current of the blood is slow and undisturbed, but quite enough when it is more rapid and forcible.

This view of the matter obtains some illustration from what has fallen under my observation in certain cases of rheumatic endocarditis, where a perfect cure has ultimately taken place. Here, before the endocardial murmur has ceased altogether, there has often been a period during which it has been sometimes absent and sometimes present—absent during repose, present on exertion. You will hardly yet be able to see the force of this argument, in which I am anticipating a part of our subject to which no allusion has hitherto been made; but you will see it and appreciate it hereafter.

Three or four cases have fallen under my notice where an endocardial murmur has arisen for the first time a few days before the patient's death. I have been quite sure that it has not existed previously, and I have had no prior suspicion of disease of the heart. In these cases the act of dying was slow and lingering. And although the murmur was declared a few days before death, it did not arise until the dissolution might be said to have already begun. But unfortunately the opportunity has not been afforded me of making inquiry into the cause of this remarkable circumstance after death. I con-



jectured that it might be owing to blood which had begun to coagulate within the ventricle, or even to inflammation of the endocardium, such inflammation (as we shall see) may take place at the very going out of life, and deposit lymph.

The following case was kindly communicated to me by Dr. Macleahlan of Chelsea Hospital:—  
“ An in-pensioner, aged 61, long affected with paralysis and other symptoms indicating organic affection of the brain, became bed-ridden about a fortnight before death. He lay in a lethargic semi-comatose state, and had great difficulty both in comprehending and in answering questions. During the last week of his existence the heart's action became inordinate, and both sounds were accompanied by a *bruit de soufflet* audible only in the præcordial region. He died on the 25th of January.”

“ On the 21st of the month, the following is the report I made of the physical signs; and, as they interested me much, I made repeated examinations, at different hours, always with the same result, the sounds varying in degree with unknown circumstances.

“ Percussion unusually clear in the præcordial region; heart's action inordinate, but regular; impulse seen as well as heard. Both sounds are accompanied with a *bruit de soufflet*, more intense with the first. There is a most peculiar, short, clear, abrupt barking sound occa-

sionally heard, which has its greatest intensity immediately behind the nipple, and extends only a little way on either side of it. This sound bears a remarkable resemblance to the yelping of a very young puppy dog. It appears to commence with the first sound; to be for an instant, and then to cease immediately. When it is loudest, it is easily heard with the ear a little removed from the end of the stethoscope. There is no fremitus, and the bruits are not audible in the larger arteries, or beyond the præcordial region. The man appears to suffer no pain; and pressure underneath the ribs, in the direction of the heart, neither affects the breathing nor produces uneasiness. The pulse is small, soft, and regular.

“ Scarcely any change took place in these signs up to the termination of the case, with this exception, that, as the heart’s action became more feeble, the peculiar yelping sound diminished in intensity and frequency.

“ On examination, forty-eight hours after death, I found nothing to account satisfactorily for the production of the sound alluded to. The pericardium was, however, unusually thin and dry, and contained much less than the usual quantity of fluid, for it scarcely amounted to half a drachm. It was not rough, nor did it present any evidence of inflammation. The heart felt firm, was preternaturally small, and the left



ventricle presented a beautiful specimen of concentric hypertrophy, the walls exceeding an inch in thickness, and the cavity scarcely admitting an ordinary-sized nutmeg. The right ventricle was in all respects natural, and the valves of all the cavities, as well as of the arterial trunks, were sound."

Here, with the appearances after death to help me, I can come to no reasonable explanation, how the endocardial murmur was produced during life. The case is very striking in all its circumstances, and well worth recording; and the best place I could find for it is this, where I am speaking of the endocardial murmur, never heard before, being first noticed among the symptoms of dissolution.

Can the heart, by the mere force of its contraction, produce a murmur exactly resembling that which proceeds from valvular impediment? Perhaps it can. A young woman, herself in perfect health, nursed an infant, to whom she was greatly attached, in an attack of hydrocephalus. The infant died, and she was seized with violent hysterical emotion. I did not see her until the nervous struggle had come to an end, and she was lying in bed apparently free from present excitement. But the heart was still contracting with excessive force, and with the loudest possible bellows-murmur. The next

day the heart was beating quietly, and the murmur was gone.

It is (I believe) a rare thing in adults, but in children common enough, for an endocardial murmur, simulating valvular disease, thus to arise from the mere force of the heart's contraction.

Again, I have found the endocardial murmur under circumstances in which it was assuredly owing to mechanical impediment encountered by the blood in its passage through the heart, and where the nature of the impediment was obvious enough; but it was not valvular injury.

Strange things happen to the heart when the chest is deformed. There is an end of our pretending now to calculate what its condition may be by listening and feeling and percussing. Its sounds and impulses and resonances, be they what they may, are now worth nothing at all as guides to diagnosis. The heart is dragged from its proper seat, and imprisoned in some strange place, and perhaps turned almost topsy-turvy by the encroachment of the vertebral column and the approximation of the ribs. And thus cramped in and hooped about with bone, at every movement it gives a jar that may be felt, and a sound that may be heard, in every part of the chest. And this sound, which is thus conveyed to a distance, is seldom the natural sound; but a loud whiz, the same in kind and the loudest in degree, which belongs to mechanical impediment from

valvular disease. And mechanical impediment there is, but valvular disease there is none. The weight and pressure, which the heart or its large vessels sustain from the hard frame-work of the chest, raise the impediment, and throw it in the way of the circulating blood.

Here the cause from which the endocardial murmur proceeds is without the heart, but *within* the body. It may be both without the heart, and without the body.

A little boy, aged eight years and a half, high spirited and vivacious, but thin and out of health, was brought to me under a suspicion of disease of the heart. Its impulse was not felt beyond the apex, but *there* it was in excess; yet there was no larger space of dulness than natural in the præcordial region. Upon auscultation, however, this remarkable peculiarity was made out. When the ear or the stethoscope rested gently upon the præcordial region, no unnatural sound whatever was heard. But when either the ear or the stethoscope was applied with such force as to cause the ribs to sink a little below their natural level, then a loud bellows-murmur sprang up. The space, at which it was heard, and not beyond it, was just so far as the mouth of the stethoscope covered, when it was placed upon the cartilage of the third rib as a centre. Below and above this spot the murmur vanished, and it was

audible neither in the course of the aorta nor in the carotids.

This case, which occurred to me five years ago, has made me watchful ever since, lest haply I might sometimes create the murmur I was in search of. And it is no needless caution where the patient is young and the frame-work of the chest is yielding. Never, indeed, the chest being not deformed, never but in this single instance have I produced a murmur simulating that of valvular disease. But very often, when, overearnest in what I was about, I have pressed too heavily upon the præcordial region, a sort of jarring sound has reached my ear, and brought with it the suspicion of disease, until setting the heart free from the weight and the restraint which I had inadvertently imposed upon it, I have at once lost the sound and the apprehensions too, which had arisen from my own awkward manœuvring.

It is well to know this possible fallacy of our own making, and so to guard against it.

But, though by pressure upon the præcordial region, I have often produced some unusual sort of endocardial murmur, never (as before stated), where the chest has been free from deformity, have I produced the murmur exactly simulative of that which belongs to valvular disease but in this one instance.

The chicken-breast, which scarcely passes for a deformity, is often sufficient greatly to alter the



relation of the heart to the walls of the chest. It often thrusts it forward, and brings its whole anterior surface in contact with the sternum and ribs. Hence in such cases the question, whether the heart be sound or unsound, becomes puzzling enough. Sound or unsound, its impulse is to be felt and seen in all the space at which it lies in contact with the chest, and the same space is dull to percussion. Extensive præcordial impulse and extensive præcordial dulness are the very signs of hypertrophy; and if to these be superadded the endocardial murmur, you have the complete signs denoting the commonest form of complex unsoundness which the heart is apt to undergo, viz. hypertrophy with valvular disease. But beware, now especially, beware, of creating the endocardial murmur by the application of the ear or the stethoscope to the præcordial region. Nothing is easier. I have done so frequently in such cases by way of experiment.

There is yet another possible fallacy imputing an endocardial murmur, and with it the suspicion of disease to the heart, where no such murmur and no such disease really exist. Here, however, the fallacy is not of our own making, but arises altogether from a perplexing coincidence of action between the heart and the lungs.

It has been said that endocardial murmurs are best imitated by modulations of the breathing and by help of the mouth. Hence it is not to



be wondered at that there should be an endocardial murmur which nearly resembles the natural murmur of respiration. The commonest of all the endocardial varieties is the bellows-murmur; and the natural murmur of respiration is only a gentler sound of the same kind, but more prolonged. Hence the morbid sound of the heart and the natural sound of the lungs are sometimes so much alike, that, if the systole of the ventricles and the act of inspiration kept time with each other, it might not be easy to determine from which of the two organs the murmur came.

And, in point of fact, I have sometimes listened and hesitated, and hesitated and listened again and again, before I could satisfy myself that a murmur which came altogether from the lungs did not in part proceed from the heart also. It has been carried with an impulse into the ear as if it came from the heart. The method of clearing up the doubt is to auscult the heart, while the respiration is suspended for a quarter of a minute.

I must here find a place for noting a certain auscultatory phenomenon, which, though it may not have struck the general observation, is frequent and familiar to my own, and has gained an importance in my eyes from the pathological conditions with which I have found it associated. To the ear it claims kindred with endocardial murmurs. But although the heart may be in-

strumental in producing it, it is not at all perceived within the præcordial region, but in a certain definite and circumscribed space beyond it.

Fancy a line drawn from the left side of the sternum along the upper edge of the second costal cartilage and continued an inch along the second rib; and another line drawn from the sternum along the lower edge of the third costal cartilage and continued an inch along the third rib. Between these two lines a space is included, in the whole or in part of which a murmur is often audible coincident with the systole of the heart, when no such murmur can be perceived either in the præcordial region, or in the course of the aorta, or in the carotids, or in any part of the arterial system, but here and here only. It is a gentle bellows-murmur, quite obvious to the ear and unmistakeable in its character.

Of such a murmur, often audible in this situation exclusively, I am certain as a matter of fact, and certain too of its very remarkable accompaniments. I have witnessed it either in those who were undeniably consumptive, or in those who were too justly suspected of being so. I cannot say in what proportion of the phthisical it occurs; but I am continually meeting with it.

Yet my knowledge goes no farther than the living symptom. I have gained no explanation of it by dissection; I have only a clinical experience of the matter. But there is a practical use-

fulness in the mere experience of coincident facts, though their pathological relation be not yet understood. Thus, where from my direct examination of the lungs I cannot get beyond a suspicion of tubercular disease, the murmur in the space indicated must always contribute to confirm it.

Supposing the pulmonary artery in its first divisions to be the seat of the murmur, does it become such in consequence of its own disease, or by reason of pressure or impediment reaching it from diseased lung?

Thus far we have dwelt upon endocardial murmurs, as the result of mechanical impediment to the circulation, real or suspected; real, when it proceeds from known valvular disease; suspected, when it proceeds from other causes which are less surely ascertained. Moreover, we have noticed, by the way, the possible fallacy in some cases of mistaking the respiratory murmur of health for the endocardial murmur of disease. And we have alluded to a murmur accompanying the systole of the heart, and heard in a certain thoracic space, and remarkable for its frequent coincidence with pulmonary consumption.

Certain endocardial murmurs yet remain to be noticed, which are quite distinct pathologically from all these. Synchronous with the systole of the ventricles, audible in the præcordial region, and extensively diffused through the arteries, re-

sembling the bellows-sound, and so having the commonest quality of endocardial murmurs, not distinguishable by the ear from those which proceed from mechanical impediment to the passage of blood, yet themselves springing from a different cause, they form a class by themselves, and a most important class it is.

I allude to the cases in which there is an unnatural sound, both endocardial and arterial, and yet no change of structure in the heart and arteries, but a change in the relative proportions of the constituent elements of the blood.

The one general fact with which the sound is constantly associated is an impoverishment of the blood, or the state in which its red globules are deficient and its serum is in excess.

Now this impoverishment of the blood would seem to stand to the endocardial murmur in the relation of a cause from observation of their constant coincidence merely; and much more so, from the observation that, upon removal of the first, the second always ceases. In proportion as under proper medical treatment the blood becomes richer, and is made to abound more in red globules, the murmur waxes fainter and fainter in the heart and arteries, until it is finally altogether inaudible in both.

But if this endocardial and arterial murmur be really owing to an impoverished state of the blood, one would expect to find that the simple



abstraction of blood to a large amount would produce it at any time in a healthy person. And so it will. We are not indeed accustomed thus to bleed healthy persons purely for the sake of experiment. But healthy persons sometimes become the subjects of such treatment in the case of accidents and injuries, and in the first access of acute inflammation; and then we take advantage of the occasion for learning the effect of the experiment beyond the purpose for which it was instituted. And so we find that, if in a healthy man we carry bleeding far enough to blanch the surface of the body, we create an audible systolic murmur in the præcordial region, and diffuse it through the arteries.

Now this murmur is prominently characteristic of certain forms of disease; and, knowing how we can produce it at will, we should expect to find nature producing it exactly or nearly in the same way. Profuse or protracted menorrhagia, by the time it has blanched the skin, has this murmur for its sure accompaniment. Here is direct loss of blood. Chlorotic anæmia has the same. Here is no direct loss of blood, but, what is tantamount to it, a defect or failure of the assimilatory functions, whence the mass of blood is not replenished in due proportion to its expenditure upon the uses of the economy.

Generally accompanying the endocardial and arterial murmur, when it is owing to anæmia or



an impoverished blood, there is another sound quite different in kind, and formed neither in the heart nor in the arteries, but traceable to the same pathological condition.

In following the murmur from the heart along the aorta and the subclavian artery, and then above the clavicle, when you reach the carotid you find a new sound superadded to it. You perceive the bellows-murmur coming and going with distinct whiffs, and keeping time with the systole of the heart in the neck as in the chest; but in the neck you perceive, moreover, *a continuous hum*, like that which reaches the ear from the hollow of a marine shell. This is a thing so evident, that it was noticed and described, and variously speculated upon by those, who first practised auscultation. But their speculations were wide of the mark. Whence or how it arose no one could tell, until the sagacity of Dr. Ogier Ward traced it to the veins, and showed it to proceed from the movement of the blood within them.\*

The vein, which offers itself most readily to the application of the stethoscope, and admits all the easy experiments which serve to certify the fact, is the internal jugular. Place the instrument upon the neck by the side of the trachæa, and pretty close to it, and at the same time rest your finger upon the space between the angle of the

\* Med. Gazette, vol. xx. p. 7.

jaw and the mastoid process; and when your ear has caught a continuous humming sound, and listened for a while and made sure of it, then press your finger firmly down upon the vein, and the sound, if it be the true venous murmur, will immediately cease; then raise your finger, and if it be the true venous murmur, it will immediately return.

A little management and address are needed, to find this venous murmur, and then keep it within hearing when you have found it. I have seen it found by accident, heard for a minute, and then lost and never heard again. The instrument has been laid carelessly upon the neck and the murmur has been audible immediately; and then, in expectation of making it heard to more advantage, the neck has been put upon the stretch, the chin raised and the head thrown back, or turned far round to the opposite side, whereupon the murmur has ceased. Then the neck has been relaxed, the head brought forward, and the chin inclined towards the sternum, but the murmur has not returned. The truth is, a very free current of blood is essential to the production of the venous murmur. A slight degree of pressure upon the vein will alter its character, and pressure very far short of that which would arrest the current of blood will abolish it altogether. And thus the neck being put upon the stretch, the muscles, which lie parallel with

the vein and across it, are made to exercise pressure enough upon it to interfere with the free current of blood, and to stop the sound; or the neck being relaxed, the vein and the integuments get folded together, and so pressure is produced in another way, and this equally stops the sound. Try different degrees of pressure upon the internal jugular vein with the stethoscope when the venous murmur is distinctly audible, and you will find how lightly you must hold the instrument to keep it constantly within hearing, how inconsiderable an amount of pressure will obliterate it, and how each degree short of that which obliterates it will give it sundry varieties, and make it musical.

Now these murmurs, whether appertaining to the heart and arteries or to the veins, which have their origin in the quality of the blood that circulates within them, furnish an eminent example of the highest degree of comprehensiveness both for knowledge and for use, which can belong to the idea of a symptom.

Where these murmurs are, there a countless variety of other symptoms is found in company with them, pointing to all organs of the body, and giving notice that the functions of all are going wrong; the surface pale and cold, palpitation and dyspnoea, appetite perverse, digestion imperfect, nutrition insufficient, secretions scanty and unhealthy, pain every where, and a shattered nervous system and an enfeebled brain. Such a

portentous crowd of symptoms strikes the observation at once. But what they all mean, we cannot tell, until we take one single symptom for their sole and sufficient interpreter. The murmur which is at the same time endocardial and arterial and venous is comprehensive of them all, and includes the knowledge of them all, inasmuch as it points directly to their one common source, even the impoverished blood. And further, this same murmur not only contains the knowledge of all the rest, but it is the single representative of them all as an indication of treatment. Standing, as it does, for the sign of impoverished blood, we treat what it denotes and nothing else. But in so doing we treat inclusively every error of function throughout the body which proceeds from it.

Such are endocardial murmurs, which have their origin and seat and efficient causes within the heart, which are different in kind from its natural and healthy sounds, and which take the place of them. Their causes (as far as our present knowledge has reached) may consist in unusual vibrations induced among the particles of the blood either by mechanical obstacles which it encounters in its passage, or (whether directly or indirectly) by a change in its constituent elements, and quite independent of such obstacles.

But there are also exocardial murmurs, of which the general characteristics have been already



given. These have their origin and seat and efficient causes *without* the heart; and, while they are different in kind from its natural and healthy sounds, they do not supersede them, or take their place, or necessarily interfere with them in any way. They are formed in the pericardium.

The lungs and the pleura, and the heart and the pericardium, have many things which bear a resemblance or analogy, so far as regards the murmurs resulting from them respectively in their several diseases; these may be now usefully adverted to in illustration of that part of our subject at which we have arrived.

Such is the structure of the lungs that they perform their natural and healthy functions with certain perceptible sounds. But such is the structure of the pleura that no perceptible sound whatever attends its natural and healthy functions. The pleural surfaces glide over each other in perfect silence, and the ear can catch not the least notice of their contact and movement in opposite directions. Hence this difference belongs to the auscultatory signs arising from diseases of the lungs and of the pleura respectively; that in pulmonary diseases the auscultatory signs consist of the *natural* sounds exaggerated or diminished, or occasionally modified, as well as of *new* sounds, whereas in pleural diseases, there being no natural sounds to be exaggerated, diminished, or



modified, the auscultatory signs consist of new sounds alone.

The same may be said of the heart and the pericardium. Take the heart apart from the pericardium, and it never moves without a sound. Take the pericardium apart from the heart, and it never moves with one. Take the diseases of the heart apart from those of the pericardium, and the auscultatory sounds denoting them may consist of its natural sounds exaggerated, diminished, or variously modified, or of sounds altogether new in kind. Take the diseases of the pericardium apart from those of the heart, and the auscultatory signs denoting them (since the pericardium has no natural sounds capable of being exaggerated, diminished, or modified) must always consist of sounds new in kind, and of such only.

Farther, the lungs and the heart have not only the elements of their diseased murmurs contained in their natural sounds, but it is by the same instrumental means that they bring out both; and these are the very instrumental means of their own vital functions. It is the air by which, and out of which, the lungs effect their office of respiration; and it is the air by which they form the sounds which are the audible notices both of their health and their disease. It is the blood by which, and for the sake of which, the heart fulfils its office of circulation,

and it is the blood by which it makes its sounds of health and its murmurs of disease.

But it is otherwise with the pleura and the pericardium. As there is no element of the to-and-fro sound discoverable in health, so there are no instrumental means then in operation out of which it could be formed. The pleura does not make its attrition-sound by the respired air, or the pericardium its attrition-sound by the circulating blood. But the instrumental means of both are purely the creation of their diseases. These consist of strange substances separated from the blood and deposited upon the pleural surfaces, or the pericardial surfaces, spoiling their natural smoothness and lubricity, interrupting their noiseless play upon each other, and causing them to grate together with a sound.

## LECTURE IV.

GENERAL ESTIMATE OF THE USES OF AUSCULTATION  
APPLIED TO THE HEART.

WHAT I have laid before you is the alphabet, or at most the spelling-book, without which you will never be able to understand the auscultatory language expressive of diseases of the heart. And, until you know it, and know it well, you must go on blundering and guessing as children do, until they have learnt to read.

But to decipher the auscultatory language of diseases of the heart easily and accurately is an affair requiring labour and use and docility. If you find it a hard task, you must not excuse yourselves upon the plea that this, that, and the other man knew nothing about it, and yet they were esteemed wise in their time; or that many men, who now pass for wise, deride it, or that many, who profess to understand it, make mistakes about it, or apply it to no good end.

Yet, as the ability to read does not make a man literary or learned, but only furnishes him the means, the indispensable means, however, of becoming so, so neither does the skill to decipher the auscultatory language of the heart make him

all at once a great pathologist or a good practitioner in respect of its diseases; but, being constantly, soberly, and diligently applied, it furnishes him with much help towards a surer knowledge and a better treatment of them. For auscultation is conversant with principles.

And above all things we should covet principles; for most certainly they do not abound in practical medicine. The records of practical medicine are chiefly made up of the sagacity of this man and the experience of that, of much that has been luckily conceived or cleverly reasoned, and of some things that have been concluded with a fair probability of truth. But in all this sagacity and experience, in all that has been so conceived and reasoned and concluded, there is wanting the test of principles to tell us how much real truth is contained.

Now, as the best men among us have ever felt the want of principles to test the truth both of their own knowledge and that of others, so they have been ever ready to accept them whenever they have appeared. Whenever in medicine any thing like a discovery has been made, any thing which has had the show of a principle or a law, a large surrender of cherished opinions has always followed, and knowledge has seemed to begin its career afresh from a new starting place. Mr. Hunter's work on the blood and inflammation abolished half the knowledge which the world had

then to boast on these subjects. It showed that there had never before been any such thing as a pathology of local morbid processes. Abounding in principles, or in the germs of principles, it afforded a point of departure for all future study and observation, to the disregard of abundance of notions, opinions, and reasonings previously accepted and allowed.

So in our own times auscultation has been a discovery in the art of clinical observation, inasmuch as within a certain sphere it has furnished us with principles really scientific for its use and exercise; consequently in regard to the diseases of those organs to which auscultation is applicable, almost all the previous records of clinical medicine have become useless. Our inquiries now begin, and begin with certainty, from a new starting point; a point which, formerly, if they ever reached, they only reached by conjecture.

For how then stood our knowledge of diseases of the heart before auscultation came to illustrate them? Truly by the profession at large they were not much thought of or inquired about until the commencement of the present century. Yet Senac's was a great work, and the Epistles of Morgagni abound in scattered information of great value upon this subject. A good deal was known, but the knowledge was hidden knowledge. It had not reached the general mind. Indeed the heart, in respect of its pathology, seemed to lie



out of the high road of popular interest, until Corvisart wrote.

The treatise of Corvisart, when I was a student, was in all our hands. And it well deserved to be; for in it there was knowledge of the best kind displayed in the best manner. Taking what information he thought valuable from the works of others, and blending it with his own special experience, he brought the whole to bear upon the pathology and clinical diagnosis of diseases of the heart. Thus the entire subject may be said to have been first brought out of obscurity, and first placed fairly within our reach, by the clear and vigorous and methodical and popular manner in which it was handled by the genius of Corvisart. I do not believe it possible that the diagnosis of diseases of the heart could ever have been carried beyond the point to which Corvisart brought it (yet how far was it from certain?) by any thing less than some new discovery in the art of clinical observation.

That discovery has been made which we possess in auscultation. Thus many forms of structural disorganisation and disease belonging to the heart, which, after much time and much calculation of circumstances near and remote, and much cautious reasoning, could heretofore be only plausibly and probably conjectured during the life of the patient, are now known at once and infallibly attested by the ear. But the ear must be a well-educated and well-practised ear, or it is

not a trustworthy witness. Remember this; for the knowledge of the senses is the best knowledge; but delusions of the senses are the worst delusions. And men are as often deceived by their ears as by their eyes; and they may *hear* ghosts as well as see them.

But it is needless to dwell upon the indisputable fact, that by the use of a well-disciplined and well-practised ear we arrive at a readier and surer diagnosis of diseases of the heart than by all the other means of inquiry which clinical observation can command. Still we should be careful to form a right estimate of auscultation; to value it for what it is worth and not for what it is not. There is much indifferent taste and worse judgment in the world, which are apt to applaud in the wrong place, and so to injure many things really good by their undiscerning patronage of them. Auscultation has suffered in this way from its friends; and therefore I think it worth while to try and set it right with the world, not concealing its weak points while I endeavour to do justice to its strong ones. I am speaking of auscultation only respective to the heart.

The age we live in cannot be better characterised, as to the manner in which medicine has been studied and pursued in it, than as the age of morbid anatomy. Almost the only instrument of pathological research has been the scalpel. Now, that great good has resulted from morbid

anatomy it would be unjust to deny. But this good there is a tendency to exaggerate ; and, such as it really is, it has certainly fallen short of what might have been anticipated from the universal study of almost half a century. At length it has now confessedly done its best ; and all great improvements in medicine pathologically or practically must henceforth be looked for not from morbid anatomy but from other sources.

So far as morbid anatomy contemplates the late or last results of disease which are fixed and irremediable and unalterable, its value is very small. But so far as morbid anatomy contemplates *disease in progress*, and scrutinises and explains its organic processes, its value is very great.

Now the objects of morbid anatomy are the same with which auscultation is peculiarly conversant in the organs to which it reaches. And auscultation must have its value estimated in proportion to these objects, be it less or be it more. When during life it announces, even with infallible certainty, the late or latest results of disease, now become fixed, irremediable, and unalterable, auscultation is at its lowest estimation. When during life it declares with equal certainty the existence of disease while it is yet active, progressive, and remediable, auscultation is at its highest value.

Of the diseases of the heart which affect its

structure, and are equally the objects of morbid anatomy and of auscultation, the great majority are tardy and secret in their growth, and by the time they are so far developed as to disclose themselves to our clinical research they have already passed beyond the reach of our remedies. They are no sooner known to exist than they are known to be incurable. Already they bear the character of results, not of operations. Atrophy and hypertrophy of its muscular substance, and dilatation of its cavities, cartilaginous thickening and ossification of its valves—these are the forms of disease or disorganisation of the heart which crowd upon us in hospitals, and are submitted to us in private, to say what they are, and how to cure them.

And indeed by help of auscultation we *can* say what they are, but cure them we cannot. While they reveal themselves almost infallibly to the ear, they are absolutely beyond the reach of any restorative power which either belongs to the body spontaneously, or is capable of being called into exercise by art. Their clinical diagnosis is indeed wonderfully complete and beautiful; and the ability of forming it which proceeds from a well instructed ear would, upon each occasion of its success, deserve to claim a sort of triumph, if success were not very common.

For a long time, after most physicians of common sense had accepted auscultation, and allowed its use,

and most physicians of moderate experience had gained some familiarity with its practice, its application to the heart was only known in cases of its fixed, unalterable, and irremediable disorganisations; and of these it certified the existence, and this was all. But there is nothing so captivating as NEW knowledge. Even though its subject be incurable diseases, which it renders not a whit the less incurable, still it is captivating. Hence the extraordinary interest with which auscultation invested these incurable diseases of the heart, simply by making us sure of their existence.

Cases of such diseases always abounded in hospitals. They were essentially difficult cases. Their symptoms were hard to interpret into any definite meaning. They betokened that in some way or other the heart was diseased, and that in some way or other their sure termination was death. Day by day to watch over these cases and to treat them was an irksome duty—it was even a thing to damp the spirits.

But auscultation brought to them a new light and a new interest. And then these same became the cases which we were continually busy about, which we were never tired of visiting and examining and ausculting, and of examining and ausculting again and again; and so comparing our clinical observation during life with the disclosures of morbid anatomy after death we became vain of our often-verified diagnosis.



Nay, we were not only captivated with, but we almost made a plaything of, our new knowledge. Every variety of sounds arising from the heart which the ear could catch and discriminate acquired a fanciful importance; and attempts were made to signify them by apt similitudes, and even to express them by musical notes.

But no man who rightly estimates the ends of knowledge could rest in this and be satisfied; for this would be to value what he knows, not by its fruits, but by his own satisfaction in knowing it.

Thus far then in respect of the heart auscultation had taught us a surer and more exact acquaintance with many diseases which still we could not cure. The physician had become wiser, but the patient had profited little.

But there is such a thing as having knowledge in reserve; such a thing as cherishing and increasing and perfecting it in hope, and looking patiently forward that the time may come when mankind shall be the better for it.

It was in this spirit that the more sober-minded of medical men at first, and for a long time, continued to exercise auscultation in its application to the heart. Full of interest about it, they were ever improving their skill in it, and ever learning from it all that it was yet able to teach. But they held their new knowledge not as a boast, but as an encouragement, believing that there

was a sealed-up treasure of usefulness within it, which they should one day penetrate and disclose.

Yet time went on : and still auscultation only told us of mischief when it was done, not of mischief while it was adoin<sup>g</sup>. Still it found a place only for the remedy which could render what *must be* borne more tolerable, not for the remedy which could come in aid of counteraction and reparation and restoration to health. Then, looking upon the purely mechanical nature of the abnormal sounds of the heart, and at the mechanical way in which they are formed, one might be pardoned for beginning to doubt whether they would ever gain a high value among the signs of diseases whose phenomena are pre-eminently vital ; of diseases, which have pain and fever and nervous irritation for their prominent ingredients ; and whether they would ever help us better to understand and better to manage those diseases, which to know early is alone to know profitably, and to treat early is alone to treat successfully ; whether, in short, they would ever be available for diagnosis and practice in the acute inflammations of the heart.

Again, one might be pardoned for suspecting, from their very nature, that the structural injury of the heart capable of producing auscultatory signs must be always of large amount, and the growth of long time, and that thus those signs

would be found in the end to annex themselves solely to its chronic diseases.

Time still went on ; and those who held the keys of knowledge in this great experiment, the physicians of hospitals, and who had the fit objects and opportunities ever at hand for proving and applying this new discovery in the art of clinical observation, were still continually aiming it, and pointing it at the same mark, viz. at the attainment of a greater practical benefit. And at length the mark has been hit, and the prize has been won. For now there is no truth experimentally more certain than this, that auscultatory signs above all others, and oftentimes before all others, and oftentimes in the place of all others, may be safely trusted to declare the beginning and the augment, the decline and the cessation of acute inflammation in those structures of the heart, which are especially, if not solely, obnoxious to it ; and that the same signs may be confidently appealed to as guides, by which to choose the remedy, and apportion its quantity and regulate its force, and continue or discontinue its application.

A great amount of structural mischief (it appears) is not needed to produce in the heart an unwonted sound. As with the more delicate pieces of machinery the least injury will produce a jar, so with the heart ; and as with them this jar is often the *first* notice of something wrong, so with the heart ; and as in them the ear will often at once

detect whether the fault be of a wheel, of a serew, or a spring, so in the heart it will at once tell whether the disease be of the membrane which invests it without, or the membrane which lines it within.

Let it then be borne in mind, that the diseases and disorganisations of the heart, which auscultation reveals to us during the life of the patient, are of two classes. The one includes those which are perceptibly progressive from day to day, and in which we contemplate a present moving energy and operation. The other includes those which are stationary, and in which we contemplate a fixed and permanent result.

Now it is upon the first class of affections of the heart that I shall principally dwell; because they are those of which there is a less familiar knowledge in the world, and therefore more need of information, and because from the newness of their matter they have a greater interest; but above all, because they are really and practically the most important. For the danger to life with which they are charged is great, and great, too, is the danger of permanent injury to the organ if life be saved; so that all the extreme vigilance of time and opportunity which we can bestow, and many of the most powerful remedies which medicine can furnish, are needed to save life and to save the organ. Besides, these are the affections of the heart which more than others seem to

belong to it as its own by a strict and peculiar, though not quite exclusive, appropriation. For in them, except where it is occasionally shared by the lungs, the whole force of morbid action is expended upon the heart. In them, except where the mischief occasionally reaches the lungs also, life is perilled or destroyed by the magnitude of the injury done to the heart; and in them, except where some part of it is occasionally called for by the lungs, the entire strength of the remedy is brought to bear directly upon the heart; well therefore may they demand the most careful inquiry which we can give them.

Upon the second class of affections of the heart, those which are secret and chronic in their growth, and unalterable and irremediable in their nature, I shall content myself with a more general commentary. Indeed, for the sake of practical utility, this part of the subject needs rather to be compressed than enlarged. It is difficult to say what is its natural limit: it runs out into so many subjects beyond itself, that if they were all pursued they would lead us a ramble over the whole field of pathology and practice.

After auscultation has taught all that can be learnt concerning these affections as inherent in the heart, their exact seat, and the exact nature of the mechanical change of structure in which they consist, there is an end of all our concern



with them as such. No treatment follows. The injury done cannot be undone. We have most probably made a correct diagnosis. It is a poor boast indeed; but it would be hard to grudge us the little satisfaction (it is all we claim) of having done so.

Whatever objects of further interest belong in any way to these cases, whatever objects of medical treatment there are in any way connected with them, must be sought and found out of the heart itself and beyond it. But in most of these cases it is not what we hear with our ears, it is not the injury suffered by the heart that directly kills. Doubtless, if we could remove it, the patient would be well; but though we cannot remove it, much more must be added to it before he dies. For out of it is apt to spring every kind of disease which can be formed by blood and blood-vessels in all parts of the body; congestions and effusions, hemorrhages and inflammations, cerebral or pulmonary, renal or hepatic. These constitute diseases of various character and various name, according to the organ or the function which they damage or destroy. Now it is of some of these that the man whose heart-disease is of chronic growth commonly dies; and these are the objects of medical treatment — objects for anticipation, for postponement, for mitigation, and for temporary cure.

Concerning auscultation, then, in its application

to the heart, our inquiry hitherto has embraced the nature of its signs and symbols, and what they are in themselves, and how they are formed, and what they directly indicate; also the certainty of their use in the discovery of diseases of this organ during life, and the extent to which clinical observation has profited by them, and wherein, according to our present knowledge, consists their greatest practical value, and wherein their least.

We will now proceed to pursue the subject in its practical details, when much of what I have stated generally will be more clearly seen and acknowledged; this indeed, and much besides, which cannot in this place be intelligibly explained, viz. that auscultation has actually disclosed to our knowledge by far the most important portion of the entire pathology of the heart. Before I enter into these details, I wish to say a few words, by way of excusing myself to those who may find fault with me for many omissions, and altogether for too much conciseness in handling so large a subject as diseases of the heart.

There are subjects in medicine about which the information of various kinds to be found in books is enormous; and if you would learn all about them that is known or surmised, all that is settled or is yet in controversy, you must go through abundance of reading, and have abundance of leisure, that so by thought and meditation you may make all that you read your own.

But many of these subjects are practical subjects. Yet, extensive and difficult as they are, there are men whom duty and conscience require to have such a knowledge of them as they can use readily and profitably every day of their lives; and such a knowledge, doubtless, many *do* possess; and surely, you may fancy, this must be a *perfect* knowledge; but perfect, as including every thing that belongs to the subject, it certainly is not, perfect in its uses it may be.

The truth is, the knowledge which rules and regulates practice, and which best insures its success, is commonly a *selected* knowledge. It is also a knowledge which is compressed and compendious in its form and compass, and so always manageable and always ready for using, and made to him who holds it surer and surer by daily trial and experiment.

Now upon the subject of the heart and its diseases, what knowledge I have to impart to you must be a *selected* knowledge; for passing by what to me, at least, is less certain and less stable, I shall be content to dwell upon what from experience is best understood by myself, and is likely to be most useful to you. Here is a great hospital; and here I hold that all teaching by lectures should have for its first and principal purpose to give effect to that self-teaching, which, from the objects which surround us, all may practise and profit by who have eyes and ears and a

docile mind. Do not believe a word that I say until you have gone into the wards and proved it. There you will find your great book of instruction. I only pretend to supply a key, a glossary, or an index to it. Use that book as you ought, and then, though in the end you and I may have the same knowledge, it will not be because it has passed from my mind to yours, but, being gained by your own observation, ratified by your own proofs, and matured by your own thought, you will have it and hold it as your own independent possession.

## LECTURE V.

INFLAMMATION OF THE HEART. — ENDOCARDITIS AND PERICARDITIS. — THE ENDOCARDIAL AND EXOCARDIAL MURMURS THEIR CHIEF DIAGNOSTIC SIGNS. — HOW THEY BECOME SUCH. — THE ENDOCARDIAL MURMUR, THOUGH FOUND IN NUMEROUS OTHER DISEASES OF THE HEART, YET CONDITIONALLY THE SURE SIGN OF ENDOCARDITIS. — THE CLINICAL KNOWLEDGE OF ENDOCARDITIS A NEW KNOWLEDGE. — ITS CONNECTION WITH ACUTE RHEUMATISM. — HOW IT CAME TO BE DISTINGUISHED, DURING LIFE, FROM PERICARDITIS.

WE proceed next to consider the diseases of the heart *clinically*; and, first, its inflammations. These, as far as we know, have their local origin and seat almost exclusively in the endocardial and pericardial membranes.

Now the entire clinical history of endocarditis, or pericarditis, viewed in all its details, is a very large and a very intricate subject; too large and too intricate to be made intelligible, unless it can be greatly abridged and simplified. But how to abridge and simplify it, and yet do neither wrong nor prejudice to any substantial truth, is a difficult matter.

For this purpose I shall set up one pre-eminent sign for each disease, and direct my course by it.



And thus the clinical history which I shall give of each will be little more than a commentary upon its one pre-eminent sign, and the time and conditions of its appearing, continuing, and ceasing; while I shall regard other signs, many or few, constant or occasional, coming or going, as subordinate to it.

The pre-eminent sign of endocarditis is the endocardial murmur, and of pericarditis the exocardial. But a clear notion must be had how they become such before we advance a step further.

The abnormal sounds or murmurs of the heart in their primary signification are, for the most part, the immediate result of causes merely mechanical, the jarring and creaking and grating of a piece of machinery that is out of order; and in learning whence and how they are produced, we learn at the same time *what part* of the machinery it is that has suffered injury.

But it is a vital piece of machinery this with which we have to do; and the injury that makes it jar and creak and grate is the *product* of vital processes, of an unhealthy kind, which are, or have been, at work within it. Thus, in learning the mechanism of the heart's abnormal sounds we learn something of its diseases — we learn their seat.

This is the first piece of knowledge that comes to us from the study of the heart's abnormal

sounds, the diagnosis of the heart's diseases in respect of *their seat*; and from the study of them and them alone this diagnosis may reach an extraordinary accuracy. Yet still it is a diagnosis only of *their seat*; and to rate it at any high value would be ridiculous, if it led to nothing beyond itself.

But besides the seat of its disease, there is their nature, their origin, their progress, their events, their treatment to be inquired into. These are great heights of knowledge, and we must in some way contrive to reach them. Yet how can the theory of its abnormal sounds, when best understood, possibly help us? At first sight it appears as the lowest round of a ladder, the step just off the ground, upon which, when a man has duly secured his footing, he seems at a disheartening distance from the top.

Well, then, how comes the endocardial murmur to be the pre-eminent sign of endocarditis, and the exocardial of pericarditis, for neither one nor the other has any thing to do with inflammation *in its essence*, or with the vital inflammatory process as such? They have only to do *with the products* of inflammation and the peculiar mechanical effects resulting from them in the situations where they are now found. Thus they become diagnostic of inflammation, not absolutely, but concurrently with other signs and with certain

present states and conditions of the constitution at large.

The endocardium becomes rough and rugged from various causes, and from inflammation among the rest ; and the pericardium becomes rough and rugged from inflammation, and scarcely (as far as I know) from any cause beside. But, from whatever cause they become such, the endocardial murmur will have the same character in one case, and the exocardial the same character in the other. For in both cases the murmurs *alone* testify to the rough and rugged surface, and nothing more. But then the whole constitution is in a state of fever, and when the present disease, which it suffers, is such as has endocarditis or pericarditis for its frequent accompaniment, then should either of the murmurs, not heard before, all at once arise, it would proclaim inflammation of that membrane, to which it pointed, without a doubt.

Thus, though the murmur *alone* would not mean endocarditis or pericarditis, yet all other symptoms put together would not be enough to make us sure of either one or the other *without* the murmur. But *with* the murmur a very few are sufficient to stamp the certainty of either.

Bearing these considerations in mind, we are prepared to enter upon the clinical history of these two inflammations with these two murmurs for our guide. We will take endocarditis first.

Of all auscultatory signs belonging to the heart, the endocardial murmur occurs the oftenest, and includes the most. It may result from any kind of injury sustained by the internal lining or valvular structure of the heart, which can raise an impediment to the passage of blood. It may result from any kind of force, within the body or without the body, yet exterior to the heart, which is so brought to bear upon it as to compress its cavities. Such are spinal curvatures and foreign growths within the chest; and such is any thing of weight, or bulk, or active power, applied upon the præcordial region, sufficient to cause the sternum or the ribs to yield. And it may result from the altered quality of the blood, from anæmia.

Now there are always within the hospital numerous examples of the endocardial murmur. Perhaps at this moment you might here find cases showing severally each of the specified modes in which it is capable of being produced. Such ready opportunities of comparing and contrasting things apparently the same, and so arriving more surely at the truth, may not always be within your reach. Therefore you should make the best of them while they are so.

In studying these cases you must not expect to find the endocardial murmur its own sufficient interpreter. From simply listening to the murmur, and asking no questions, you will not be able to tell what it means. In the man, who a few days

ago was seized with fever, being then in perfect health, it means one thing; in the man, who has been out of health for years and has been long suffering dyspnœa and palpitation, it means another; in the man deformed from his birth, another; and in the pale chlorotic girl, with the feeble, frequent, jerking pulse, it means another. Yet it is the same kind of murmur, the murmur simulating the sound of bellows, in all; and being the same, it cannot entirely explain itself—it cannot be its own interpreter. It requires aid from concomitant circumstances to decide its meaning in each particular case. The aid it needs is often very little; but that little it must have, and then it tells its story clearly and explicitly.

Should it happen, when great heat and nervous excitement had been newly developed throughout the body, and when actual inflammation had recently become visible on external parts; should it happen that the endocardial murmur, well known never to have been present before, *then* arose for the first time; assuredly it would at once fix the seat of disease in the endocardium, and determine that disease to be recent and acute inflammation. Though there might yet be no pain, no palpitation, no direct symptom whatever referrible to the heart but the endocardial murmur alone, yet it alone would be enough to fix the seat, and, under the circumstances, the nature of the disease.



Truth in all its kinds is most difficult to win ; and truth in medicine is the most difficult of all. And here is the proof of it. This acute inflammation of the endocardium, this endocarditis, has been (I will not say) altogether unknown to me, but unknown in its real extent and frequency during two-thirds of my professional life.

Morbid Anatomy had sought for it, but had sought at random, and so had only found it once or twice by chance. It had put a few cases upon record, showing that such and such were the appearances which characterised this *rare* disease, when it happened to occur. And clinical observation was equally in the dark : it had stumbled upon it now and then by accident ; but when or where to look for it, or by what signs to recognise it, it could not tell.

Now clinical observation, though never blind, was, until lately, always deaf. Yet there were always many diseases which during the life of the patient spoke only to the ear. These could never have been known, or could only have been guessed at, until clinical observation learnt the use and exercise of that sense by which alone they are discerned.

Hence Auscultation has had the effect of making it appear that *rare* diseases have all at once become strangely multiplied, whereas it has only disclosed what was before hidden, and made that

the subject of sure diagnosis which was before hit upon by chance.

Endocarditis is one of these apparently new diseases; and it will be interesting to trace in what manner our knowledge and sure diagnosis of it is due to Auscultation.

Endocarditis is at present chiefly known as a concomitant of Acute Rheumatism.

In the year 1826 I was the first to teach the students of this hospital the fact, that whenever the heart was affected in acute rheumatism, a sound different from the sound of health always accompanied its contraction. This was then a new fact, and one of immense importance; and all succeeding observation has gone to confirm its truth. This sound in the vast majority of instances was the bellows' murmur. I must not call it endocardial, for it was not yet known to be so. But in some, instead of the bellows' murmur, it was some strange sound difficult to describe; and in others this strange indescribable sound and the bellows' murmur seemed to occur together: there was a mixture of both.

My notion *then* was, that all these sounds arose in some way or other out of inflammation of the pericardium; and taking them all, severally and in combination, as the signs of pericarditis, I was amazed to find how far the frequency of its occurrence in acute rheumatism exceeded the common calculation and belief.

In process of time I found, upon a comparison of cases, that where in acute rheumatism the bellows' murmur occurred *alone*, the affection of the heart was upon the whole far less severe and far less perilous to life, than where some other unnatural sound occurred *alone* or in combination with it. I observed, too, that in some cases where the bellows' murmur was unequivocal, the patient betrayed no uneasiness, no palpitation, in short, no other symptom which could give the least suspicion of a diseased heart; yet that in the great majority of instances where it once existed it remained permanent as long as the patient continued under my care and observation.

At length I began to doubt whether the bellows' murmur arising in the course of acute rheumatism was really derived from the pericardium, and to suspect that it proceeded from the internal lining. But for years the practice of this great hospital did not afford me a single opportunity of resolving my doubt, or of confirming my conjecture. For of that disease of the heart, which, coming on during acute rheumatism, is characterised by the bellows' murmur, no patient of mine ever died, and I could learn nothing about it from dissection. But what my own experience would not furnish, M. Bouillaud's has supplied. Many have died during the active progress of this disease under his care, and dissection has found it to be inflammation of the endocardium.

Thus we are indebted to M. Bouillaud for our first knowledge of this important fact.

Nearly about the same time Dr. Watson and Dr. Stokes of Dublin further illustrated this important subject by separating *those other* sounds of the heart, which I have mentioned to occur in acute rheumatism, from the bellows' murmur, and analysing them apart. These they found to possess the character of attrition, as if produced by surfaces moving to and fro upon each other, and traced them home to their local origin in the pericardium, and showed the condition of their production to be a defective lubricity, or a ruggedness and unevenness of that membrane, such as would result from inflammation. In short, they showed these to be the proper signs of pericarditis, as the bellows' murmur is of endocarditis; that, when in acute rheumatism either occurs alone (as it often does), the disease is simple pericarditis, or simple endocarditis; and that when in acute rheumatism both occur together (as they often do), there is a mixture of the two diseases in the same subject.

The bellows' murmur coming on in the course of acute rheumatism is a sure sign of inflammation of the endocardium. Here, then, we will drop this exceedingly vulgar name, and call it "endocardial" again. But observe, it is the general character of the morbid actions predominant in the system at large which determines

the particular character of the local disease out of which the endocardial murmur arises. *They* are inflammatory, and *it* is inflammation.

In endocarditis, besides the endocardial murmur, there may be other symptoms present directly referrible to the heart, or there may not. There may or may not be pain. There may or may not be an excessive impulse, or an intermittent, irregular, or fluttering action of the heart. But the fact of endocarditis is not rendered more or less certain by their presence or absence.

There may be both pain and palpitation; yet endocarditis cannot be surely inferred to exist, unless there be the endocardial murmur withal.\* There may be neither pain nor palpitation, yet endocarditis cannot be inferred not to exist, if the endocardial murmur alone be present.

Seeing, then, that the endocardial murmur alone can determine the existence of endocarditis, you are required to search after it in every case of acute rheumatism. I say emphatically *to search after it*, because it is one of those signs which must always be sought before it can be found. It does not intrude itself upon our notice like palpitation, or an irregular pulse. The patient does not draw our attention to it as he does to pain.

\* I do not say that it would not be fairly suspected, and that it would not be right to act as decidedly upon the suspicion in such a case as upon the matter of fact. It certainly would.



The physician must make it out entirely for himself. And indeed it is infinitely important that he should have the earliest possible notice of it with a view to the earliest possible application of the remedy.

Never omit, therefore, to listen to the præcordial region whenever you visit a case of acute rheumatism, and visit a case of acute rheumatism oftener perhaps than you otherwise would do merely for the sake of so listening. All may seem to be going on well. The general symptoms may be far from severe. The chest may be free from pain. The heart's action may not awaken suspicion by its force or irregularity. Nevertheless, its internal lining may be inflamed, and, if you listen, the endocardial murmur may convey the momentous fact directly to your ear.

## LECTURE VI.

ENDOCARDITIS CONTINUED. — ITS GENERAL DESCRIPTION LESS USEFUL THAN A CLINICAL COMMENTARY UPON ITS INDIVIDUAL SYMPTOMS. — THE ENDOCARDIAL MURMUR, IN ENDOCARDITIS, IS SOMETIMES PRECEDED BY A CERTAIN ROUGHNESS OR PROLONGATION OF THE HEART'S NATURAL SOUNDS. — OFTEN IT ARISES ABRUPTLY. — SEAT AND DIRECTIONS OF THE MURMUR. — ITS ACCOMPANIMENTS, PAIN, ABNORMAL IMPULSES, AND ACTIONS OF THE HEART. — THEIR PRACTICAL VALUE, AS SYMPTOMS, ACCORDING TO THEIR DEGREE, AND ACCORDING TO THE DIFFERENT RELATION WHICH THE ENDOCARDIAL MURMUR BEARS TO THEM IN DIFFERENT CASES. — THEIR DIVERSITIES OF RELATION CONTAIN INTIMATIONS RESPECTING THE STAGES AND PROGRESS OF THE DISEASE. — THE SAME CONFIRMED BY THE SUCCESS AND FAILURE OF REMEDIES. — THEIR PRACTICAL IMPORTANCE.

ENDOCARDITIS is one of those diseases which do not admit of general description. For what is meant by the general description of any disease? It is the display of its symptoms *collectively*, sometimes by strictly copying after nature, and sometimes using so much of artificial arrangement, as, while it goes neither beyond nor contrary to nature, may serve to make the whole more intelligible. Some such descriptions, when they have conveyed the truth with great force

and faithfulness, have been regarded with the same sort of pleasure with which we look upon a well-drawn picture. But, after all, they are more pleasing than profitable. Perfection in this kind was reached ages ago, yet we go on describing what has been better described before, and are venturing with rash hands still to retouch the master-pieces of Aretæus.

But of those diseases, which have been the most perfectly described, we have not the most perfect knowledge. The physician's knowledge can then only be called perfect, when outward signs make him sure of what is going on within, and offer some plain and indubitable mark for the direction of his remedy. This knowledge, however, does not come from dwelling with satisfaction upon graphic pictures of diseases, nor from seeking to take in *all* their symptoms, and viewing them at large, but from meditating on the meaning that is wrapt up in a few.

What remains for me to say of endocarditis will be in the way rather of clinical commentary than description. I shall take its auscultatory symptom, and a few symptoms beside, and comment upon them, almost one by one, and show what is the value that belongs to each in teaching us the realities of the disease and the use of remedies.

Whenever, then, those conditions of disease arise which are apt to involve the heart, *i. e.* in

all cases of acute rheumatism especially, listen to its sounds, and note in them the least change from what is natural. There may be yet no endocardial murmur. But its systolic sound may be unnaturally prolonged, or it may be unnaturally rough. Now I hardly ever knew an instance of acute rheumatism in which such unnatural length or roughness of sound, as a practised ear could well discriminate and detect, has not become an unequivocal endocardial murmur in twenty-four hours.

Those who have been accustomed to attend me in my visits to the wards of the hospital, know how often and how truly from these peculiarities in the sounds of the heart, which I have noticed for the first time to-day, I have anticipated that the murmur would be fully formed to-morrow. Experience indeed has taught me to regard them as its almost certain preludes; and thus, under the circumstances, they have become to me almost as certain signs of endocarditis as the endocardial murmur itself. I no sooner perceive them than, without waiting for the murmur, I begin the treatment of endocarditis at once.

It happens commonly, however, that the natural sound of the heart is changed into the genuine endocardial murmur, without notice or prelude. Now the murmur may be heard at the apex chiefly, or at the basis chiefly, or in both situations equally. It may, moreover, be heard

in the course of the aorta and of the carotid arteries; or in the course of the pulmonary artery, and not in the aorta, or in both simultaneously; thus denoting that the seat of the inflammation may be either in the auriculo-ventricular valves or in the semilunar valves; either on the left side of the heart or on the right, or on both sides at the same time.

It has been already said that the murmur being coincident with inflammatory actions in the constitution at large, determines the disease of the endocardium to be inflammation, and that standing alone, and being the only sign directly referable to the heart, it determines the fact as surely as it would in company with a dozen other signs immediately pointing to the same organ. And in truth it sometimes *does* stand alone, and to it alone you must needs trust; and you may trust implicitly.

Often, however, it does not stand alone. Other symptoms directly referrible to the organ are superadded; pain and anguish of various degrees and kinds, excessive impulse, intermittent, irregular, faltering, fluttering action of the heart.

Now are these symptoms to be altogether rejected and passed by? Have they nothing whatever to teach in this matter, after the endocardial murmur has already taught what the disease essentially is? On the contrary, they have a great deal to teach, and therefore they are to be



highly prized and made much of. In every disease seek to come at the purely diagnostic symptoms if you can, and put a high value upon them. But do not imagine that other symptoms have no value at all. You learn the disease in its essence and seat from its diagnostic symptoms. But other symptoms commonly tell you of its magnitude, and of its probable event; other symptoms sometimes become the guide of its treatment.

The endocardial murmur fixes the seat of the disease without a doubt, and conditionally determines its nature. But we gain a surer measure of its degree, and of the peril involved in it, from the pain and anguish referred to the heart, and from its disturbed and embarrassed actions.

When pain is present, the time of its first occurrence varies much in different cases. In acute rheumatism I have known patients, when questioned, admit that they suffered pain, or make voluntary complaint of it unasked (pain produced or augmented by inspiration, pain produced or augmented by pressure), in the præcordial region, while yet the ear has detected no sound of the heart which determined the seat or nature of the disease. And such pain I have known to cease altogether, without being followed by the auscultatory signs of either of those diseases of the heart which so frequently accompany acute rheumatism, endocarditis, or pericarditis. But from such pain experience would lead me to anticipate

that the auscultatory signs of one or the other would quickly follow; and prudence would lead me at once to begin the treatment of the anticipated disease.

I have mentioned a certain length and roughness of sound to which my ear is accustomed, as a frequent prelude to the endocardial murmur of endocarditis. Præcordial pain may be added to this length and roughness of sound; and, when it is, the murmur is so sure to follow, that it would be folly to delay the treatment of the disease until it arrives.

Præcordial pain occurring thus early, and after the manner specified, may serve at least as a salutary warning of what we are to expect and to be prepared for. And salutary, indeed (I am persuaded), it has sometimes been, when it has led us to act upon a strong anticipation of the disease instead of waiting for the authentic sign of its actual existence. For thus, if we have begun the treatment only a single day sooner than we otherwise should have done, we may have perfectly cured the disease which, but for the gain of this single day, would never have been more than half cured. The gain of a single day in the treatment of endocarditis is a gain indeed!

But the præcordial pain may not arise until the endocardial murmur has already informed us what disease we have to deal with, and we have already taken our measures for its cure. From

the præcordial pain *thus* occurring, I do not see what new suggestions of treatment can be gained. But many new suggestions may be gained from it (as I have already intimated), according to its degree and its kind, respecting the disease itself and the peril which it involves.

In endocarditis some patients say nothing of any pain they suffer until they are asked about it, and others complain of it unasked; while there is nothing in the countenances of either that tells you that they are suffering pain. Pain of this amount need not disturb your calculation of the result.

Others at once betray their pain by the countenance; and being questioned about it, will speak of an indescribable anguish, which they refer to the præcordial region. This is its centre, and hence it radiates; but it has taken possession of the whole nervous system. Now pain or anguish of this kind (call it what you will) deserves to be taken into serious account. It denotes that the disease has already got the springs of life within its grasp, and that going on to increase it must kill.

Just as the pain, when it is superadded to the murmur in endocarditis, gives intimations of a more or less formidable disease, so do the actions of the heart in like manner, according to the degree in which they are baffled or disturbed.

Some excess of the heart's impulse is a very

common symptom of endocarditis. It often precedes the murmur in point of time, and often it rises simultaneously with it, and no sooner do you hear the one than you feel the other. It is apt to occur very early.

Not so common as excess of impulse, but still not an unfrequent symptom of endocarditis; not always so very early, but still not a late symptom of endocarditis, is an intermittent action of the heart. I think where the intermittent action occurs, it will be found generally, perhaps always, to follow an excess of impulse; and then both will exist together in the same case.

But neither from mere excess of impulse, nor from the intermittent action of the heart, if the intermissions be not frequent, need any great apprehension arise. With the advantage of early treatment, the intermittent action commonly soon subsides, and the excess of impulse not so soon, but not long afterwards, leaving the murmur alone. Upon these symptoms, however, while they remain, attention must be always steadily fixed. For, if the case have not the benefit of early and efficient treatment, or be in its own nature intractable, they are changed into disordered actions of another kind, and of formidable import: the excess of impulse passing into extreme feebleness of contraction; and the rare intermissions into small, unequal, irregular flutterings.

By the time the movements of the heart are



brought to this condition, that dreadful præcordial anguish, which has been described, has already appeared; and presently the whole vascular system, and the whole nervous system, and every organ and every function which supports life, are baffled and overwhelmed. Then come orthopnœa, and lividity, and threatened suffocation, and impossibility of sleep, and collapsed features, and jactitation, and delirium, and death.

In this manner may endocarditis, announced and specially characterised by the endocardial murmur, and marked in its progress by one bad symptom after another, run on rapidly to a fatal termination. But hitherto, with all the experience of the disease which this hospital has afforded me during sixteen years and more, I have never witnessed it end thus.

But I believe such an event may and does happen, upon the credit of those who *have* witnessed it. And I believe it the more, from having myself occasionally seen a state of things which manifestly tended towards it, but which never reached it, yet which served to show me how possible it would be for endocarditis to be fatal in its acute stage.

Therefore, in every case, as soon as the murmur announces that the endocardium is inflamed, I consider that I have surely a serious, and possibly a fatal, disease to deal with; and I employ without



delay the remedies upon which experience has taught me to rely for arresting its progress.

Nay, more, so important do I consider the gain of time in the treatment of endocarditis, that (as I have already said) I deem myself justified in acting upon a strong expectancy of the disease, before the murmur has yet unequivocally declared it.

The diversities of relation which the endocardial murmur is found to bear to other symptoms belonging to the heart in various cases, are well worth a little farther notice and consideration. For from remarking these diversities, and calculating together with them the success or failure of remedies, according to the time and conditions of their application, I am led to conclude that the endocardial murmur also bears, in different cases, a different relation to the actual stages and progress of the disease itself—a fact which, if it be true, must have important practical bearings.

I believe then, 1st, that in some cases of endocarditis the murmur is coincident in point of time with the very commencement of the inflammation; 2dly, that in some, and those the most frequent, cases it does not arise until the inflammation is somewhat advanced; 3dly, that in some, and those the least frequent, cases it does not arise until the inflammation is on the decline, or has actually ceased.

1st. The coincidence of the murmur with the

commencement of the inflammation seems *thus* denoted. Upon a review of cases, I find that it was often the first symptom detected. The patients had hitherto suffered no pain or uneasiness of the præcordial region, no palpitation, no dyspnœa. But the murmur being once heard, pain, palpitation, and dyspnœa, one or all of them, quickly followed.

Here then, if ever, the murmur marks the beginning of the disease; since, if the disease existed prior to the murmur, it must have existed for a time without any symptom at all. The fact is rendered more probable from these further considerations. It was in these cases that medical treatment, promptly applied, was oftenest successful; and it was in these cases, more frequently than in any other, that the murmur altogether ceased under the use of remedies, and so afforded the best evidence we can have of a perfect cure.

2dly. The postponement of the murmur to a somewhat more advanced stage of the inflammation seems denoted thus. Upon a review of cases, I find that in the majority of them the murmur was preceded by other symptoms more or less referable to the heart—such as pain or anguish, palpitation or dyspnœa; and that an interval of from one day to a week was apt to elapse between the first appearance of such symptoms and the subsequent accession of the murmur.

Now, although the symptoms enumerated could

only direct suspicion to the heart, and had they passed away without any accession of the murmur, it must have remained doubtful in what manner the heart had been affected, or whether it had been affected at all, yet the murmur, when it at length arrived, became the sure interpreter of all that preceded it. It declared the other symptoms to proceed from the same disease as itself, — viz. endocarditis; and that this disease had existed as long as they had existed, and for some time before it became audible.

The effects of remedies in these cases, both by their success and their failure, pointed distinctly to the same conclusion. There were among them examples of perfect cure; these were the cases in which the murmur was preluded for a day or two by præcordial pain, palpitation, and dyspnœa *coming on while the patients were already under medical observation*. These, as soon as they appeared, served as signals to direct the remedies to the heart. Thus the treatment of the endocarditis was instituted in anticipation of the disease, before its authentic sign had yet arisen and determined its undoubted character. I say, in anticipation of the disease—I ought rather to say in anticipation of our own certain knowledge of it; for the murmur after the lapse of a few days arose, and thus distinctly marked the nature and seat of the disease. But in a few days it ceased,

and thus distinctly marked the perfection of the cure.

Again, among the cases belonging to this class there were many of imperfect cure. These were those in which the murmur was preluded for a longer time by præcordial pain, palpitation, and dyspnœa, which *came on before the patients were yet under medical observation*. It was not uncommon to find in the subjects of acute rheumatism that præcordial pains and palpitations had existed for two or three days before their admission into the hospital, and yet there was no murmur; and that these præcordial pains and palpitation would still continue for two or three days after their admission, ere any audible roughness or murmur was detected. Here the treatment was instituted at the earliest period possible under the circumstances, and was still beforehand with *our knowledge* of the disease; but it was too late for the *disease itself*. At length the murmur arose, showing what all the previous symptoms meant, and declaring the disease, both what it was and where it was, and sanctioning its conjectural treatment. But having arisen, it never ceased, and thus distinctly marked the imperfection of the cure.

These facts hardly leave it doubtful, that endocarditis and all its essential morbid processes, as well as the opportunity of its treatment, are often comprised within a period prior to any audible murmur; that even within this period the disease

begins and advances, and often proceeds so far as to do an irreparable injury to the endocardium; and that within this period the opportunity of its treatment must be promptly seized to be successful, and that, if tardily used, it will even then fail.

3dly. That sometimes the murmur does not arise until the inflammation is on the decline, or has actually ceased, seems very probable from the following considerations. There were a few well-watched cases in which this happened. During the progress of the rheumatic fever and the patient's confinement to bed, no murmur was audible; but when the fever and the rheumatism had ceased, and the patient had left his bed and was walking about the ward, and was already deemed convalescent, then the murmur was for the first time audible. In these cases, any previous symptoms which could intimate a possible suspicion of the heart being affected were very slight, and had yielded to slight remedies, or no such symptoms were either noticed or treated at all.

The endocardial murmur arising under these circumstances was unchanged by medical treatment. It remained as long as the patients continued under observation.

The inference from such an event is clearly this,—that an inflammation of the endocardium had accompanied the rheumatic fever; that this



inflammation was of small activity, and insufficient during its progress to interfere with the natural sensations and movements and sounds of the heart, but enough in the end to produce by its effects some permanent inequality on the surface of a valve, and a permanent murmur as the sign of it.

Now, if the foregoing facts be true, and the conclusions from them be just, they will help us greatly in estimating the real value of the murmur as a sign diagnostic of inflammation of the endocardium, and as a guide for its treatment.

And its value must vary exceedingly in both respects, according to the period of the inflammation at which it becomes audible. When the murmur is itself the first symptom, or among the first symptoms, of endocarditis, then it has its highest possible value both for diagnosis and treatment. It makes us sure of the disease as soon as it begins to exist. It makes us bestir ourselves for its cure when it is most within reach of a remedy.

But when the murmur is not audible until the endocarditis is considerably advanced, and until it has already been preceded for some days by other symptoms, doubtless it is still diagnostic of the disease, and is still a guide to its treatment; but its practical value in the individual case is diminished. For now, did we wait for the murmur to tell us what the disease is and how to treat it, it would inform us indeed at last with more certainty than

any other sign; but we should come in with our knowledge and with our remedies when they were too late. But we do not wait. Our experience of the frequency with which the murmur follows such and such symptoms referable to the heart, arising in the course of acute rheumatism, makes us regard them as its sure precursors, and makes us act as if it were already present, and begin the treatment of endocarditis while it is yet not quite certain whether the disease be endocarditis or no. Here the value of the murmur is indeed very great, in a certain point of view. Its value is not shown by the use we make of it for the diagnosis and the treatment of the very case in which it occurs, but by the use we make of our foregone experience of it in other cases, which we are now turning to such eminent profit in this. The murmur, when it at length arises in the individual case, only serves to show, that the conjecture which we formed from the more equivocal symptoms, concerning the existence of endocarditis, was right, and that the treatment instituted upon that conjecture was right also.

Further, when the murmur is not audible until the endocarditis is on the decline or has actually ceased, it is of no use whatever, either for the diagnosis or treatment of the disease as an inflammation. The inflammation is gone by, and the murmur denoted nothing about it during its progress. But it is diagnostic of its effects, which remain and are permanent. Whatever use and

interest it now may have, they belong to it as marking the commencement of a series of pathological changes which are yet to come. For it ascertains beyond a doubt the time, and the nature, and the seat of the first rudiment of disorganisation, which will in the end probably involve the whole heart.

## LECTURE VII.

PERICARDITIS. — THE EXOCARDIAL MURMUR ITS PRE-EMINENT SIGN. — AN IMPERFECT MURMUR SOMETIMES PRECEDES THE TRUE. — IN PERICARDITIS, AS IN PLEURISY, ANOTHER AUSCULTATORY SIGN BESIDE THE MURMUR, DULNESS TO PERCUSSION. — THEIR RELATION TO EACH OTHER NOT EXACTLY THE SAME IN THE TWO DISEASES. — IN PERICARDITIS OTHER SIGNS IMMEDIATELY REFERABLE TO THE HEART, BESIDES THE AUSCULTATORY : VIBRATIONS SENSIBLE TO THE TOUCH, UNDULATIONS TO THE EYE. — OTHER SYMPTOMS OF PERICARDITIS. — THEIR RELATION TO ITS AUSCULTATORY SIGNS. — FROM THESE OTHER SYMPTOMS, AND CHIEFLY FROM ITS KNOWN CONNECTION WITH ACUTE RHEUMATISM, PERICARDITIS OFTEN RIGHTLY PRESUMED TO EXIST, AND OFTEN SUCCESSFULLY TREATED ; YET OFTEN OVERLOOKED, AND OFTEN TREATED TOO LATE, AND OFTEN FATAL, FOR WANT OF THE AUSCULTATORY SIGNS.

BEFORE the exocardial murmur was made out and verified, and clearly discriminated from all other sounds referable to the heart, there was no certain diagnosis of inflammation of the pericardium. The subject is one in which I had myself taken a peculiar interest, and had done my best to gain accurate information. But I am now fully aware, that, for a series of years, half the cases at least which I regarded as inflammation of the

pericardium were in fact inflammation of the internal lining.

Surely a lasting debt of gratitude is due from mankind to those who shall discover an unerring sign of any disease. The more so, if the disease be of a formidable nature. The more so still, if the sign declare the disease early enough to bring our knowledge of its existence fairly within the period that will allow it to be successfully treated by medicine. All this may be confidently predicated of the sign in question and of the disease it indicates — of the exocardial murmur and inflammation of the pericardium: and our debt of gratitude is due to Dr. Watson and to Dr. Stokes of Dublin; for these two eminent physicians pursued their enquiries independently and successfully to the same result. Among the great variety of auscultatory signs referable to the heart in its different states of disease, they discriminated the attrition-sounds and set them apart from all the rest, until finally, by experiment, they referred them to their proper source, and fixed the conditions of their production in the pericardium.

But there are other sounds which, prior to experience, would hardly have been suspected to proceed from the pericardium, but which nevertheless really do so. These sounds cannot, like the ordinary exocardial murmur, be imitated by any simple device that I am acquainted with. They are said to be like the crumpling of parchment,



the creaking of shoe-leather, the churning of milk. And I must leave them to be represented by these similitudes without attempting to describe them; for I am quite sure that by no description I could give, and by no similitude to which I could refer you, would you be at all the better able to recognise the sounds when you should first hear them. You must wait till you hear them yourselves; and then you will most likely be puzzled until you are told what they are.

Any one remembering what has already been said of its similitude to the sound produced by rubbing the hands together, or the cuffs of your coat, or two pieces of strong paper; also of its sensible nearness to the ear when it is applied to the præcordial region; also of its conveyance to a distance over the chest, and its non-conveyance in the course of the aorta and carotids; any one from this description would at once recognise the true exocardial murmur on first hearing it, and pronounce that the disease was pericarditis.

But (what for want of better names I must call) the crumpling, creaking, churning sounds require to be more frequently heard, before they are familiarly known and recognised. Yet an acquaintance with them is most important and most necessary. For they as surely indicate the presence of pericarditis as do the more perfect ordinary exocardial murmurs.

One fortunate circumstance, however, belongs

to these indefinite and less perfect sounds, which greatly diminishes the chance of evil that might be supposed to arise from a doubtful or postponed decision respecting their exact import as diagnostic signs. It is this:—wherever any one of them arises in consequence of pericarditis, it becomes changed, in the course of one or two days, completely or partially, into the genuine exocardial murmur. It is either merged entirely in it, or partakes enough of its character to leave its import no longer doubtful. And then the genuine murmur being once established continues such thenceforward through the whole course of the disease, or only changes its character a few days before its final disappearance.

Touehing these indefinite sounds referable to the heart in acute rheumatism, there is another circumstance which is worth a particular notice. It is this—that they will sometimes come and go for two or three days, and then cease altogether, or then become permanent.

For an important fact, not prominent in itself, it is often difficult to gain the attention it deserves, unless you invest it with the circumstances of actual practice. Let me do so for the fact in question. This then has several times happened. A clinical clerk, having charge of a case of acute rheumatism, has distinctly heard an unnatural sound at a certain space of the præcordial region, and two or three other students have heard the same as distinctly as himself. The space has been

a small space, and they have marked it with a circle of ink that they might the more easily find it again. At my visit to the hospital some time afterwards my attention has been called to the fact; but no such sound could I hear, and those who have heard it before have confessed that they could not hear it now. But I have returned to the same patient in half an hour, and then I have heard it, and everybody else has heard it distinctly and at the very spot indicated.

Now, these sounds of an indefinite kind, belonging to acute rheumatism, which are restricted to a small præcordial space, and are now audible and now inaudible several times a day for two or three days together, and then either become constant or entirely cease, always proceed (I believe) from the pericardium. For, when they become constant, they gain the character of the ordinary exocardial murmur, and spread themselves widely over the præcordial region or beyond it.

In the cases where the sounds ultimately cease without becoming either more pronounced or constant, and without occupying a larger space, it is reasonable to believe the disease still to have been pericarditis, but of small degree and extent.\*

\* I would not willingly represent the diagnosis of the seat of disease within the heart by the quality of its abnormal sounds, as more easy, or more constant, or more absolutely sure, than it really is. Notice is hereafter (page 143.) taken of certain cases, in which it could not be determined whether the inflammation was of the endocardium or of the pericar-

In connection with the more or less perfect exocardial murmur, many circumstances remain to be noticed which are of great interest, and which serve further to illustrate the pathology and diagnosis of inflammation of the pericardium.

You recollect the history formerly given of the attrition-sound occurring in the course of pleurisy<sup>\*</sup>; how it arose at one period of the disease, ceased at another, and returned again at a third. The solid products of inflammation, the lymph, upon the opposite surfaces of the pleura, first produced the sound. The fluid products of inflammation, the serum, in the cavity of the pleura, obliterated it. And the absorption of the serum from the cavity, while the lymph still remained upon the surfaces, caused it to return.

Now in pericarditis there are the fluid products of inflammation as well as the solid. There is serum as well as lymph. And the signs of fluid effused within the pleura and the pericardium are the same. The fact of its existence, and the measure of its accumulation within the pericardium, can only be known by the degree and extent to

dium. Dr. Benée Jones informs me, that at St. George's Hospital he has sometimes found no distinction possible between the endocardial and exocardial murmurs themselves; and that then the variations of sound, from day to day, and even from minute to minute, and often in consequence of change of position, have afforded him the means of recognising inflammation of the pericardium.

<sup>\*</sup> In the course of lectures given at St. Bartholomew's, those on pleurisy preceded those on diseases of the heart.

which the præcordial region, and perhaps some space beyond it, may be dull to percussion. Thus in pericarditis dulness sometimes occupies a part and sometimes the whole of the præcordial region; sometimes it reaches beyond the præcordial region, as high as the second, and even the first left rib; sometimes it extends beneath the whole length of the sternum, except about an inch at the top, and even beneath the cartilages of the ribs on the right side.

Surely, then, this dulness to percussion is a most important sign, and hardly inferior to, and hardly less diagnostic of, the pathological conditions to which it points, than the exocardial murmur itself.

But do these two, viz. the murmur and the dulness, bear the same relation to each other as signs of disease within the pericardium, as they have been seen to bear as signs of disease within the pleura? In pleurisy the attrition-sound and the dulness are never coincident, but are always found to supersede each other, one ceasing as soon as the other arises. Is this the case in pericarditis? My own experience would answer almost absolutely, "No!" As soon as I have discovered the exocardial murmur at any part of the præcordial region, so soon have I almost always found dulness to percussion. And, to whatever extent the dulness to percussion has spread beyond the præcordial region, the murmur has accompanied it, even



as high as the first left rib, and beneath the sternum, and far beyond it, even to the juncture of the cartilages with the right ribs. Further, I have known dulness of the præcordial region to be the *first* sign, and to subsist several days *alone*, and yet the attrition-sound has at length been superadded to it, when they have thenceforth been continued together. In pericarditis, then, this I take to be the *general* truth, namely, that the murmur, which is produced by lymph, deposited upon the surfaces of the membrane, is neither abated, nor abolished, nor otherwise altered in its character by the serum effused within its cavity. It is not, however, the *universal* truth.

Indeed, I have seen a few instances of pericarditis, where the murmur, clearly heard day after day, has at once become very indistinct, or has been suddenly lost altogether. The disease has been at its greatest activity, and the heart at the very time has suffered some extraordinary bafflement of its actions. But the sound, thus abated or abolished, has suddenly returned, and in a day or two has been as perfect an exocardial murmur as it was before, and then the heart's actions have recovered their regularity. In these instances the remedy which has thus instantly relieved the heart and restored the sound has been a large blister vesicating the whole præcordial region, and a wide space around it.

Now dwell a little thoughtfully upon these instances, and upon the several attendant circumstances, both when the sound goes and when it returns, and above all, upon *the remedy*, and its immediate effect; and then say what they can denote, but now a sudden increase, and now a sudden decrease, of fluid within the pericardium?

But still, if the common experience be in accordance with my own, and if my own speak the truth in this matter, the fact will be, that serous effusion within the pleura *always* obliterates the attrition-sound, and that serous effusion within the pericardium *generally* leaves it unaltered. Now there must be some way of accounting for so different a result from conditions apparently the same. The following considerations, perhaps, point out a way that is plausible at least.

Fluid within the pleura exercises its pressure upon the most yielding organ of the body, the lungs. They make no resistance, but, shrinking in their dimensions, and giving place more and more as the fluid increases, they recede further and further from the ribs, until at length they are forced into an incredibly small compass by the side of the vertebral column, impervious to air and useless, but in themselves perfectly free from disease.

But fluid within the pericardium produces no such effect upon the heart by its pressure. The heart, of all organs of the body, is the least yielding. It is the pericardium that now yields. But then, in the greatest accumulation of fluid within it, the space intervening between the pericardium and the heart will bear no comparison with that between the pleura and the lungs in ordinary cases of pleurisy. Further, when in pericarditis death has taken place while the inflammation was progressive, dissection has generally found as much solid as fluid matter within the pericardium, and sometimes even more lymph than serum. The heaped-up, curdled albumen covering the entire heart, and adherent to, or pendulous from, the loose pericardium, would make it appear that the interposed serum (which itself contains flakes of lymph) was not enough to prevent the opposite surfaces from touching and rubbing against each other, and so to prohibit the murmur.

In the few cases where the dulness to percussion supersedes the murmur, or greatly alters its character, it is probable that the fluid products of inflammation exceed the solid, that the serum exceeds the lymph, in an unusual measure.

The exocardial murmurs of more and less perfect character, and the dulness to percussion of the præcordial region,\* and of more or less space beyond it, are strictly auscultatory signs, being both

equally learnt and appreciated by the ear. It is with such that I am now principally concerned. But I may be permitted perhaps to conjoin with them two other signs, directly referable to the heart, indicating the same conditions of disease and often found in their company, but learnt and appreciated by an appeal to other senses.

In pericarditis, while the præcordial region is dull to percussion and the exocardial murmur is heard, an undulating motion often becomes visible to the eye in some of the spaces between the cartilages of the ribs on the left side. It has always been either between the cartilages of the second and third ribs, or of the third and fourth, or between both at the same time, that I have seen this motion, and never in any other situation.

So, too, in pericarditis, while the præcordial region is dull to percussion and the murmur is heard, a vibratory motion often becomes sensible to the touch in some of the spaces between the cartilages of the ribs on the left side. As I never *saw* the undulatory, so I never *felt* the vibratory, motion elsewhere than either between the cartilages of the second and third, or of the third and fourth ribs, or between the cartilages of both simultaneously.

The vibration (I believe) is the more frequent of the two, and often occurs unaccompanied by any visible undulation. But the undulation was never apparent to my eye without my finger

being able to detect a sensible vibration at the very same spot,

It is hardly necessary to give a formal explanation of these phenomena. It is no unusual thing that the same vibrations which convey sounds to the ear should make themselves sensibly felt by the touch, or that a fluid should impart its own undulatory motion to contiguous bodies.

Now these two signs, which address themselves respectively to the sight and to the touch, are simple, direct, and easily apprehended, but inferior in value, both to the murmur and to the dulness on percussion, as pointing out inflammation of the pericardium. Neither the vibration to the touch nor the undulation to the eye are always present. Many a case of pericarditis has passed through its entire course without either one or the other manifesting itself. So that, if we depended altogether for our diagnosis upon either or both, pericarditis must often go undiscovered.

Again, where they *do* appear, it is not (as far as I observe) ever at a very early period of the disease. So that, though they might afford a sure diagnosis enough, it would be less practically valuable as coming late, when the time for the most efficient treatment of the disease is past.

But fortunately our diagnosis of pericarditis need never depend upon them; and more fortunately still, our diagnosis is already settled before



they appear. For in pericarditis (as far as I have observed) they never occur but as accompaniments of the exocardial murmur and the præcordial dulness. And farther, when they do occur (as far as I have observed), they always appear later, and cease earlier, than these do.

There remains another observation to be made in calculating the just value that belongs to one of the two signs in question, viz. the vibration conveyed to the touch between certain intercostal spaces. It is occasionally present in more diseases of the heart than one ; in disease of the semilunar valves, whether of the pulmonary artery or of the aorta, as well as in inflammation of the pericardium. Which disease it indicates, and in which situation, must be determined by the concurrent circumstances of the particular case.

It would not then perhaps be unjust to conclude that the exocardial murmur and the præcordial dulness are supreme in the diagnosis of endocarditis ; and that they neither receive nor require any aid from other signs directly referable to the heart, though such (it appears) there are, which are as simple, and as plainly cognisable by other senses as themselves are by the ear.

Thus far I have been dealing with these auscultatory signs analytically ; setting them apart, and describing what they are in themselves, explaining the mode of their production, and the conditions out of which they arise, and comparing them with

others that might seem to hold competition with them. And perhaps I have thus made them as intelligible to you as in this manner they can be made. You may now understand their proper sphere of diagnosis, and their just value *upon the whole*.

But, for the great uses they are to serve, you must become acquainted with them in their accustomed complications, and mixed, as nature mixes them, with the events and circumstances of actual practice.

My own experience of pericarditis is mainly derived from what it is, as an accompaniment of acute rheumatism. I have seen the disease, indeed, under other circumstances. But it has been very seldom; so seldom, indeed, that I have little acquaintance with other conditions external or internal conducing to it. I can neither tell whence to look for it nor when to expect it, except when it occurs as a part of acute rheumatism.

The pericarditis, which is acute and rapidly progressive, and, unless arrested by timely and effectual treatment, full of peril, this is the pericarditis I mean. And with this the practice of a large hospital has rendered me familiar by presenting me every year with numerous instances of it in alliance with acute rheumatism. But, separate from acute rheumatism, even the practice of a large hospital does not present me with more than an instance or two of it in several years. And, as of the disease itself, so of all the

symptoms and auscultatory signs by which I learn its existence and direct its treatment, my chief knowledge comes from acute rheumatism.

In analysing the sources of our knowledge, let us be just in allowing to all times and to all methods of investigation their due merit. It ill beseems that impartiality which ought especially to characterise every enquiry after such truths as we are engaged in, to be bent upon depreciating the labours of the past age and exalting those of the present, or disparaging old methods of research and praising new ones. Pathology and practical medicine had assuredly made some respectable advances before we were born, and before physicians had found out all the uses of their ears, and of the stethoscope.

Long before auscultation was practised, physicians knew a great deal about inflammation of the pericardium. They knew well the pathological conditions of the constitution at large out of which it is most prone to arise, and consequently when to expect it. They knew well when to infer its existence from such symptoms as were within their reach. They knew well how to treat it, using all the same remedies that we do now, and directing them to fulfil the same indications. Finally, they knew well all its consequences. Long before auscultation, pericarditis had a good claim to be considered one of those diseases which was tolerably well understood.

Now, that which served our predecessors as the basis of all they knew clinically concerning pericarditis was the general fact of its alliance with acute rheumatism. This fact, so sure and well authenticated, gave an interpretation and a meaning to many equivocal circumstances, and placed them, for the time, almost in the rank of diagnostic symptoms.

The direct symptoms from which they inferred its existence were these; — pain in the præcordial region, often augmented by pressure, anguish, and oppression of breathing, and an irregular or intermitting action of the heart. But these symptoms are not very precise; they have been found in other diseases of the heart, and in diseases of other thoracic organs. It may seem strange, then, that physicians having only these to guide them should be able to detect pericarditis so often and so surely as they did. But then (I repeat) they knew the conditions under which pericarditis was apt to occur; and, when those conditions arose, they were perpetually upon the watch for it. And thus when, in the midst of acute rheumatism, there arose a præcordial pain, an anguish of respiration, and an irregular action of the heart, they interpreted them to denote pericarditis, and they were generally right.

They were right, *when these signs occurred in acute rheumatism*, in concluding them to mean pericarditis; but, *when in acute rheumatism not*

*one of these signs occurred*, and the pericarditis existed nevertheless, they overlooked the disease; and they could not help but overlook it. And when (what more frequently happened) these signs occurred indeed, but not until the pericarditis had already existed many days, they did not overlook it, but they gained too late a knowledge of it, much precious time having been already lost to its treatment. 'Thus pericarditis has indeed been entirely overlooked by myself, and by better men than myself; overlooked when we were most upon the watch for it, under circumstances most conducive to it, even as the accompaniment of acute rheumatism. The inflammation has been severe, and, being unarrested by any remedy, it it has run on rapidly to its fatal termination; and after death we have stood amazed' at the disease disclosed to our eyes by dissection.

These are events of past years. In such unfortunate cases there was neither præcordial pain, nor respiratory anguish, nor irregular pulse; and the auscultatory signs of pericarditis were as yet unknown. But the like mistakes could hardly occur *now*; it is scarcely possible that pericarditis coming on just at the suspected time, and just under the suspected circumstances, could *now* be overlooked. Every prudent physician, I presume, searches after it day by day with his ear in all cases of acute rheumatism; and though the heart itself show no vital consciousness of its ailment



either by feeling or function, by pain or palpitation,—though the organs in closest relation with the heart, the lungs, feel nothing, suffer nothing, and declare nothing, and so there be no dyspnœa, yet will the *mere mechanism* of the disease proclaim the fact of its existence to the ear.

By reason of its perfect lubricity, the healthy pericardium carries on the movement of its surfaces upon each other in perfect silence. It is enough to make their movement audible if inflammation do but spoil this perfect lubricity, and as soon as inflammation produces ruggedness and inequality, the movement is accompanied by harsh sounds.

Still, of an acute and severe rheumatic pericarditis running on to its fatal termination, absolutely unattended from first to last by any symptoms except the auscultatory, the examples, I believe, are very rare; few at any time have died of it without any attempt to save them. The victims of an undiscovered and untreated pericarditis are few; but the examples (I am persuaded) are by no means rare of an acute and severe rheumatic pericarditis progressive for many days, and unattended in the meantime by any but auscultatory symptoms, other symptoms, however, arising at last; and many such cases (I am persuaded) were fatal *formerly*. The best treatment commenced as soon as the only symptoms then understood had declared themselves, came too late; and many such cases would be fatal *now*, did not the first

attrition-sound from the præordial region call into instant use the remedies by which we deal with pericarditis.

My experience tells me, that in acute pericarditis the fluttering, faltering action of the heart, and, with it, the respiratory anguish, are almost sure to occur, but that the time of their occurrence is almost always late, and that the murmur and the præordial dulness always precede them.

But my experience tells me, that in acute pericarditis the pain, if it occur at all, almost always occurs early. The first access of inflammation generally produces it as well in the pericardium as in other parts. Yet, early as is the pain, the murmur is often earlier.

But of all symptoms mere pain is the most inconstant and uncertain, whatever be the disease. It is so in pericarditis. It is present in one case, and absent in another strangely and unaccountably. I have known much pain, when the disease has been of little severity, of short duration, and of easy cure: and I have known the severest pericarditis pass through all its stages without pain. All other symptoms have been present to mark its reality and its progress: the murmur and the præordial dulness, and the fluttering heart, and the respiratory anguish. And sometimes the patient has died, and sometimes he has escaped by a tardy and precarious conva-

lescence. But from first to last there has absolutely been no pain.

Do not be surprised at this. Pleurisy may exist without pain; even acute, rapid, pus-effusing pleurisy. Peritonitis may exist without pain; even acute, rapid, pus-effusing peritonitis. And so, too, if in pericarditis there is sometimes no pain, it fortunately happens that there are other signs by which we can fix our diagnosis of the disease equally well without it.

See what a strange, unequal, and uncertain light pain is found to throw upon diagnosis and treatment! We find it where we do not look for it, and look for it where we do not find it. Its presence is no sure proof, its absence is no sure negation, of disease.

But still pain is a most important symptom. Where there is pain, we should always think of disease, always search after disease, and always require strong circumstances to convince us that disease does not exist. Where there is no pain in a part suspected of disease, we should never on that account conclude it to be healthy, and never be content until we find other circumstances to convince us that it is really so.

## LECTURE VIII.

THE FREQUENCY OF ENDOCARDITIS AND PERICARDITIS, OCCURRING SEPARATELY OR TOGETHER, AS THE ACCOMPANIMENTS OF ACUTE RHEUMATISM. — PRESENT RESULTS OF ENDOCARDITIS OCCURRING ALONE — OF PERICARDITIS OCCURRING ALONE — OF BOTH OCCURRING TOGETHER. — DIFFICULTY OF GAINING KNOWLEDGE OF THEIR ULTIMATE RESULTS, WHEN THE CURE IS IMPERFECT.

BETWEEN the years 1836 and 1840, both inclusive, there occurred under my care at St. Bartholomew's Hospital 136 cases of acute rheumatism :

Of these 136 patients 75 were males, and 61 were females ; of the 75 males the heart was affected in 47, and unaffected in 28.

Of the 47, the seat of disease was the endocardium alone in 30 ; the pericardium alone in 3 ; and both the endocardium and the pericardium in 7. And, while the heart was undoubtedly affected in 7 others, the exact seat of its disease was uncertain.

Of the whole number of males in whom the heart was thus variously affected 3 died. And in these 3 the pericardium and the endocardium were both inflamed.

Of the 61 females the heart was affected in 43, and unaffected in 18.

Of the 43, the seat of disease was the endocardium alone in 33; the pericardium alone in 4; and both the endocardium and the pericardium in 4; and the exact seat of the cardiac disease was doubtful in 2.

Of the whole number of females in whom the heart was thus variously affected none died.

The account of males and females taken together will stand thus:—

Cases of acute rheumatism	-	136
Heart exempt in	-	46
Heart affected in	-	90
Seat of disease in the heart:—		
Endocardium alone in	-	63
Pericardium alone in	-	7
Endocardium and pericardium in		11
Doubtful in	-	9

Deaths 3. In all of whom both the endocardium and the pericardium were affected.

Here are momentous facts which go (I suspect) a good deal beyond the ordinary notions entertained by medical men of this matter. It is believed that among the sufferers of acute rheumatism an individual now and then unluckily has his heart inflamed. The thing is looked upon as an accident which, if not very rare, yet is not very common. But it appears, from the event, not of a dozen or twenty cases merely, but of a



number large enough to furnish the measure of what naturally belongs to the disease, that as many as two-thirds of those who have acute rheumatism also suffer inflammation of the heart.\*

Further, the pericardium is popularly regarded as the special and most frequent seat of the inflammation which takes its rise from acute rheumatism. But it appears from cases sufficiently numerous, that endocarditis occurs nine times in acute rheumatism, for pericarditis once; that simple endocarditis constitutes more than two-thirds of all rheumatic cardiac affections, and simple pericarditis only one-thirteenth; and that pericarditis is more frequently found in combination with endocarditis than alone.

\* It has been suggested to me that, in the records of my practice at St. Bartholomew's, there would probably be found a somewhat greater frequency of endocarditis and pericarditis, as the concomitants of acute rheumatism, than is usually observed; and that this might be owing to the sedulity of my clinical clerks, who were ever on the alert to gain admission into my wards of (what were esteemed) interesting cases, and that thus I might get more than my share of rheumatisms in which the heart was affected. I cannot exactly tell how this may be, but I hear all physicians of public hospitals speaking of the heart being affected in acute rheumatism with a frequency far beyond the common belief.

The proneness of the heart to inflammation in rheumatic fever may not be at all times the same. It may belong to (what Sydenham would call) the epidemic constitution of a certain series of years.

Next we come to the results of these cases. And there are many results worth inquiring about ; but, when the disease is inflammation, and the part affected is vital, everybody first thinks of the great result in present death or recovery, and asks what are the hopes of life and what the fears of death.

Now, you already know that out of the 90 cases of cardiac disease occurring in the course of acute rheumatism, cases of simple endocarditis, and cases of simple pericarditis, and of both mixed, and of some doubtful, only three deaths are recorded. Well, then, have all these pains and all this care of diagnosis been thrown away upon a class of diseases which, albeit they are of a vital organ, prove fatal only in one case out of thirty ?

But besides the great result of present death, or recovery from the present inflammation, there are other results, practically and pathologically important in the highest degree, which auscultation and auscultation alone has enabled us to trace and to appreciate.

The results of simple rheumatic endocarditis were these : —

Of the 63 patients who suffered simple endocarditis in the course of acute rheumatism, 30 were males and 33 were females, of whom none died. And if the sort of subjects we have to deal with in a London Hospital be considered, their different habits and constitutions, which are

bad in the majority, and the disadvantages of their postponed treatment, so common in the acute diseases of the poor, it must be confessed that the endocarditis of acute rheumatism does not involve much immediate peril of life, when of all the cases of this disease, 63 in number, which occurred in the course of five years, not a single one was fatal.

But of these 63, whom the endocarditis did not kill, and who, as far as general symptoms could be trusted, might be pronounced convalescent or well, auscultation still told us that, after the inflammation had ceased, the membrane recovered its complete integrity of structure only in 17, and that it remained permanently injured in 46. For of the 30 males, the subjects of rheumatic endocarditis, the endocardial murmur ceased entirely only in 8; while it remained, after they were convalescent, and as long as they continued under observation, in 22. And of the 33 females the endocardial murmur ceased entirely only in 9; while it remained in 24.

Thus, while inflammation arrested and life saved in all the cases which occurred, even 63 in number, do indeed sufficiently testify how small is the present peril of life from rheumatic endocarditis, yet the entire restoration of the endocardium to its perfect structure in 17 only, and the permanent injury done to it in 46, denote a most fearful disease in regard to its distant results. For the

probability is as great as four to one, that inflammation befalling the endocardium will become the rudiment of disorganisation to the entire heart.

The results of simple rheumatic pericarditis were these : —

Of the 7 who suffered simple pericarditis in acute rheumatism, 3 were males and 4 were females, of whom none died. Life was saved in all. Inflammation was arrested in all ; and all resumed the general conditions of health. Neither, after inflammation arrested and life saved, did it happen to any one of these cases of pericarditis, as it did to 46 out of 63 of endocarditis ; not in a single case did the exocardial murmur remain after convalescence to denote a still abiding change of structure in the pericardium, as the endocardial murmur had remained, and denoted permanent disorganisation of the endocardium in so many cases of endocarditis.

But were all these cases of pericarditis perfectly cured? After the inflammation ceased, was there no remnant of injury in any case where there was no exocardial murmur to denote it?

In pericarditis, when the exocardial murmur entirely ceases, we have not the same strong grounds for believing that the pericardium has both lost its inflammation and recovered its healthy condition, as we have in endocarditis, when the endocardial murmur ceases, that the endocardium is quite sound again. These two

auscultatory signs, referable to the internal and external membrane of the heart respectively, cannot be taken equally to imply the same things both when they come and when they go. When they come, they both equally denote new matter deposited, the one upon the endocardium and the other upon the pericardium. But when they go, they do not both equally denote that the newly deposited matter has been removed. When the endocardial murmur ceases, it does denote as much; when the exocardial murmur ceases, it does not. For the endocardial murmur could not cease, while the lymph still remained on the endocardium to produce the obstruction to the blood which caused it. But the exocardial murmur could cease, while yet the lymph remained on the pericardium. For its opposite surfaces might by the same lymph be made to adhere, and thus the cause of the murmur would be removed.

Hence, therefore, out of the 63 cases of simple endocarditis, in which inflammation was arrested and life saved, I feel absolutely certain that the cure was perfect in 17; a small proportion, indeed! while out of the 7 cases of pericarditis, in which inflammation was equally arrested, and life saved, I have no assurance whatever that the cure was perfect in a single one.

But although in these and in all cases of acute pericarditis there is nothing *certain* beyond the immediate result of the treatment in arresting



inflammation and saving life, there may be something *probable* as to the condition in which the inflamed parts are left, and as to the perfection or imperfection of their cure.

What is probable, then, is this, that, whenever the pericardium is acutely inflamed, and lymph enough is deposited upon it to produce the exocardial murmur, the cure seldom amounts to a complete restoration of its natural structure, but that the whole, or some considerable portion of its opposite surfaces, permanently adhere. This is probable from the nature of the disease itself, and of the serous membrane which it implicates. And it is much more probable from what actually occurs in many cases. For after the exocardial murmur has long ceased, and the patient is deemed convalescent, signs directly referable to the heart will often remain or arise, showing that the organ is not at ease—that it still sustains an injury which baffles and restrains the freedom of its natural actions. The signs, taken alone, are not enough to define the injury, either what it is, or where it is; but taken in connection with the previous disease they are quite enough. They denote that the permanent injury has the same seat as the previous disease, viz. the pericardium, and that it consists in that change of structure to which inflammation of the pericardium naturally tends, viz. adhesion of its folds.

In 4 of the 7 cases of simple pericarditis, I

find the following records made just before the patients passed from under my observation, and left the hospital:—1. “Sounds of the heart not distinct one from the other.”—M. 31. 181.

2. “Sounds of the heart as if muffled.”—W. 22. 4.

3. “Increased impulse of the heart for a month after the exocardial murmur had ceased, and as long as the patient remained under observation.”—W. 25. 121.

4. “Præcordial region presents a greater extent of dulness to pereussion than natural.”—W. 26. 121.\*

The results of rheumatic endocarditis and pericarditis combined in the same subject were these:—

Of the 11 in whom endocarditis and pericarditis were combined, 7 were males and 4 were females. Out of these, inflammation was arrested, and life saved in 8; and 3 died. Of the 8, who were convalescent from this double disease, one of the structures inflamed, the endocardium, underwent perfect reparation in 2, for the endocardial murmur entirely ceased; and imperfect reparation in 6, for the endocardial murmur continued. As to the other structure

\* The letters and numerals here and elsewhere denote the volumes and pages of my own MS. case-books. I am aware that they can now afford no help to the reader, since they do not refer to documents open to his consultation. Still at some future time the cases may be recorded, and then the references will be of use.

inflamed, the pericardium, although the exocardial murmur ceased in all, it is doubtful whether its reparation was perfect in any. Probably there remained a greater or less extent of permanent adhesion.

Thus of these 8 cases of double disease, or of inflammation involving the two structures of the heart, I am not sanguine enough to believe that the organ recovered a perfectly healthy condition in a single instance. For in the 2, where, doubtless, the endocardium was perfectly restored, the pericardium probably adhered; and in the 6, where, doubtless the endocardium was permanently injured, the pericardium probably adhered also.

In the 3 fatal cases, the auscultatory signs denoting inflammation of the endocardium and pericardium were well marked, and on both membranes dissection disclosed the recent effects of inflammation, when it is arrested in its mid progress by death.

I subjoin an account of them which you may take as specimens of the disease: —

In one case the folds of the pericardium were universally adherent, but were easily separated. The connecting lymph was peculiarly vascular over the left ventricle, and being detached, discovered some spots upon the surface of the heart which looked like pus. The endocardium bore marks of inflammation on both sides of the heart.

On the tricuspid valve, at a little distance from its free edge, was a spot, one third of an inch in diameter, pink in the centre, and surrounded with a white elevated border. On the mitral valve were small pearly bodies, about the size of millet seeds, fringing its free edges. The aortic valve was thickened and of a pinkish colour, and displayed upon its surface, a little below its free edges, bodies of the same form and size as those found upon the mitral valve. The general bulk of the heart and the capacity of its cavities were natural, and its muscular substance had the appearance of health.

Here the exoeardial murmur ceased twelve days before the patient's death. But the endocardial murmur continued, until the symptoms of dissolution arose, and the movements of the heart were too feeble to make it audible.—M. 25. 29.

In another case the pericardium adhered at no part, but was every where covered, both on its free and reflected surface, with curd-like lymph, which was accumulated in the largest quantity over the auricles, and on the basis. The adventitious membrane being turned back, discovered red spots beneath of extravasated blood like petechiæ. The quantity of fluid in the pericardium was very small, not exceeding two or three drachms. It was like whey. The endocardium showed the effects of inflammation on both sides of the heart. The tricuspid valve had

minute pearl-like bodies deposited near its free edges. The mitral valve had bodies of the same kind in the same relative situation, and around some of them there was a slight blush of red. On passing the finger from the ventricle into the aorta, a palpable obstruction was felt in the situation of the valve. This was found to arise from a single fibrinous deposit upon one of the valvular processes. It was of an irregular form, and one-third of an inch in size at its greatest diameter. All the three processes were slightly thickened, and of a pinkish colour, but upon one only was there any morbid deposit. The general bulk of the heart and the capacity of its cavities were natural, and its muscular substance was apparently healthy.

Here the exocardial and endocardial murmurs continued, until the symptoms of dissolution arose. For four days before death, the heart could hardly be heard or felt, and all distinction of sounds was necessarily lost. — M. 25. 69.

In the third case the pericardium was only partially adherent by round loose bands of lymph, and its surfaces where they were free were covered by a continuous layer of lymph, half an inch thick, and studded with rough, unequal, villous prominences. The pericardium contained about three ounces of whey-like fluid. The endocardium bore marks of inflammation only on one side of the heart. The mitral valve had a



rough fringe of minute deposits near the edges of its free border, and each process of the aortic valve had a fringe of the same kind, only thicker. The muscular substance was perfectly healthy. The whole organ seemed rather large; but it could hardly be said that any part of its substance exceeded its natural bulk, or any one of its cavities their natural capacity.

Here the exocardial and endocardial murmurs continued until the death of the patient. — M. 31. 140.

In contemplating endocarditis and pericarditis united in the same subjects, do we find ground for believing that either naturally tends to produce the other? In 4 of the 11 cases the endocardial was prior to the exocardial murmur; and in 4 the exocardial was prior to the endocardial. In 1 case they arose simultaneously after the patient's admission into the hospital, and in 2 they were found already co-existing at the time of admission.

Such were the results of rheumatic inflammation of the heart in the 90 cases which fell under my observation in the course of five years. They include the events of life and death during the progress of the inflammation, and the conditions of perfect and imperfect cure in which the endocardium and pericardium were left after its cessation. Here my observation stopt. And, indeed, in the 17 cases of endocarditis terminating

in perfect reparation, and the 3 of complex endocarditis and pericarditis terminating in death, it embraced the entire disease. But in all the rest there were other results yet to come which my observation could not reach. These are such as are yet to emerge sooner or later in the course of future existence. The injured valves, or the adherent pericardium, or both combined, lay their own conditions upon the continuance of a man's life, and sometimes settle beforehand the manner of his death.

Sad, indeed, but most interesting and instructive, would be the entire history of the lives and deaths of all those in whom I here witnessed the first attack of disease, which spoiled the perfect structure of the heart. Such an entire history I shall never know. I may learn a few particulars of one or two whom I may chance to meet with, and this is all I expect.

But our knowledge of chronic maladies, which last for years, is not gained in the same manner as our knowledge of acute diseases, which last for a few days or a few weeks, viz. by watching their progress from beginning to end in the same individual patients.

Our knowledge of chronic maladies is picked up piecemeal from numerous cases seen for a while, and only for a while, at different periods of their progress. One case shows us the disease at its beginning, another at a more onward stage,

another at its mid period, another towards its decline, another at its end; and then, joining together the facts so collected from *many* individuals, we get our notion of what the disease may be in its entire course and character, from first to last.

Thus, of chronic injuries of the valves of the heart and chronic adhesions of the pericardium, and their results, you must not expect that I can give you the same sort of account, or attest it by the same sort of experience as I have just given and attested of the acute diseases from which they sometimes spring. I have spoken of endocarditis and pericarditis, and their results in 90 cases which I had watched from first to last. From first to last, however, was in them a period of a few weeks only. But surely I cannot speak in like manner of chronic injury of the valves of the heart, and of chronic adhesions of the pericardium, and give results drawn from an observation of 90 cases from first to last. Yet have I seen ninety, and many more than ninety, individuals, who have suffered chronic valvular injury, or chronic adhesion of the pericardium. I have seen, indeed, an infinity of patients; but in each patient I have not seen the whole disease, but only a fragment of it, and generally a very small fragment. Such single cases, in their entire clinical history from first to last, occupy years; some two or three, some five or ten, some even twenty or thirty;

and of these the conditions of medical experience can only allow a partial observation. Nevertheless, such partial observations in process of time sum themselves up to a large amount of knowledge, and furnish collectively a tolerably complete clinical history of the diseases in question.

## LECTURE IX.

INFLAMMATION OF THE LUNGS ACCOMPANYING ACUTE RHEUMATISM, EITHER ALONE, OR IN COMBINATION WITH ENDOCARDITIS, OR WITH PERICARDITIS, OR WITH BOTH.

WHEN our business is with a mere dead specimen of morbid anatomy, it does not signify how exclusively we view it. We may take it apart, and look at it through a microscope, and dissect and inject it and macerate it; and thus we may learn all that can be learnt about it. But when we are concerned with a living specimen of disease, if we would understand it, we must deal with it after a different manner. The living disease, while it works its own changes in the part it occupies, gives and receives influences and impressions to and from other parts, and to and from the constitution at large. Therefore, in order fully to understand it, our inquiries must be enlarged in proportion to it. They must reach as far as it reaches. They must not settle upon the one single object, but be carried into many things beyond it.

Our present subject is the heart and its diseases; and what we have now especially in hand to illustrate is the inflammation of its lining and



investing membranes which accompanies acute rheumatism. But the heart is not the only vital organ liable to suffer inflammation in acute rheumatism. The lungs may suffer also; the lungs, in all the several structures of which they are composed. And the diseases which result are bronchitis, pneumonia, pleurisy.

Knowing, then, the relative dependency of function between the two organs, and finding both ready to suffer alike from the same pre-existing or co-existing disease of the constitution, however our present business may be professedly with the one, we must not refuse to let in whatever light may be reflected upon it from the other.

It is not possible to make too much of those diseases of the heart which arise out of rheumatism. But it is very possible to make too little of the diseases of the lungs which acknowledge the same origin. The truth is, we have done so. The very habit of dwelling long and minutely (as we needs must if we would understand them) upon the facts which concern the pathology of the one organ has brought us unconsciously to regard it as a single centre of disease much more than it really is. It is well to be aware of so natural a bias towards error, and carefully to guard against it. Let each fact be made to carry with it the full force of its own truth, and yet, in relation to other facts which are as true as itself, let it hold

no higher value, place, or proportion, than nature has given it.

Inflammation of the heart is incident to acute rheumatism, and so too is inflammation of the lungs. The former is of more frequent, the latter of more rare occurrence. Of 136 cases of acute rheumatism, the heart was inflamed in 90, or in two thirds of the whole; while the lungs were inflamed only in 24, or one in 5½.

But that inflammation of the lungs, notwithstanding its comparative infrequency, is a matter of no mean importance in connection with acute rheumatism, will be seen from the form and character it bore in the 24 cases.

I use inflammation of the lungs as a general expression for inflammation of any pulmonary structure, either for bronchitis, pneumonia, or pleurisy. The 24 cases in question were made up of 4 of bronchitis, 18 of pneumonia, and 2 of pleurisy.

Now a bronchitis, a pneumonia, or a pleurisy involves much or little peril according to its circumstances. But here, whichever disease occurred, it always put on a serious character, either from its mere magnitude and extent, or from its force of morbid action, or from the stage at which it ultimately arrived. In the four instances of bronchitis the affection was no mere catarrh, but an inflammation largely diffused through both lungs, producing deep oppression and dyspnoea.

Of the two pleurisies, one was single and the other double. The single pleurisy produced a large effusion into one side. The double pleurisy produced a double hydrothorax. Of the 18 instances of pneumonia, in 9 the disease was of one lung, and in 9 it was of both.

Pneumonia, if it be severe and abiding, generally includes more than its name implies. How much more we can often only suspect, but not exactly tell, during the life of the patient, so entirely do the pneumonic symptoms transcend the symptoms of the concomitant disease, and in effect obscure them. Severe pneumonia will often veil a pleurisy as severe as itself. It was suspected of doing so in several of the cases in question; and, in one case, dissection at last disclosed double pneumonia with double hydrothorax.

Such forms of pulmonary inflammation are portentous ingredients in the clinical history of acute rheumatism, and give a fearful interest to it. But what if this fearful interest be further augmented by its frequent coincidence with inflammation of the heart?

Of the 136 cases of acute rheumatism which form the basis of our inquiry, inflammation of the lungs was found in 24. Here the proportion is about 1 in  $5\frac{1}{2}$ . But *how* were these 24 cases distributed? What proportion of them occurred where the heart was unaffected, and what proportion where the heart was inflamed? And, again,

what proportion, where the inflammation was of the endocardium alone; what, where it was of the pericardium alone; and what, where it was of the endocardium and pericardium simultaneously?

Of the 46 cases of acute rheumatism in which the heart was unaffected, the lungs were inflamed in 5. Here the proportion is as one to nine. But of the 90 cases in which the heart was inflamed, the lungs were also inflamed in 19. Here the proportion is more than one in five.

The 19 instances of inflammation of the lungs were distributed among these 90 cases of inflammation of the heart in different proportions, according to the part of the organ affected.

Of the 63 cases of endocarditis the lungs were inflamed in 7. Here the proportion is as one to nine.

Of the 7 cases of pericarditis the lungs were inflamed in 4. Here the proportion is more than one half.

Of the 11 cases of endocarditis and pericarditis simultaneously, the lungs were inflamed in 8. Here the proportion is more than two thirds.

What can we, or what can we not, conclude from this enumeration of facts? What general truths do they declare?

We cannot conclude, from inflammation of the lungs being found in one case of acute rheumatism out of nine, that acute rheumatism has any strong natural tendency to inflame the lungs. Neither can we conclude, from its being found in one case

of rheumatic endocarditis out of nine, that rheumatic endocarditis has a special natural connexion with inflammation of the lungs. The probability of inflammation of the lungs arising out of acute rheumatism is small; and the probability is not at all augmented by its alliance with endocarditis. For in acute rheumatism, inflammation of the lungs does not occur more frequently when the endocardium is inflamed, than when the heart is entirely exempt from disease.

We find it to be between inflammation of the pericardium and inflammation of the lungs, and between inflammation of the endocardium and pericardium occurring simultaneously in the same subject, and inflammation of the lungs, that frequent coincidence seems to establish a natural connexion.

That inflammation of the heart which is least perilous in itself is least liable to have its danger augmented by an union with inflammation of the lungs, viz., endocarditis; while that which is most perilous in itself is most frequently accompanied by inflammation of the lungs, adding immensely to its danger, viz., simple pericarditis, or endocarditis combined with pericarditis.

Of rheumatism without affection of the heart there were:—

Cases 46. — Lungs affected in 5.

Single pneumonia (fatal)	-	-	M. 26.	26.
Single pneumonia	-	-	M. 27.	81.



Single pneumonia	-	-	-	W. 21. 111.
Diffused bronchitis ending in double pneumonia	-	-	-	M. 30. 65.
Diffused bronchitis of both lungs	-	-	-	W. 24. 149.

Of rheumatism with endocarditis there were:—

Cases 63.—Lungs affected in 7.

Double pneumonia	-	-	-	M. 27. 227.
Double pneumonia	-	-	-	W. 25. 42.
Double pneumonia	-	-	-	W. 26. 65.
Diffused bronchitis passing into double pneumonia	-	-	-	W. 20. 216.
Single pneumonia	-	-	-	M. 27. 142.
Diffused bronchitis of both lungs	-	-	-	M. 24. 178.
Bronchitis passing into inflammation of the larynx and trachæa	-	-	-	W. 21. 130.

Of rheumatism with pericarditis there were:—

Cases 7.—Lungs affected in 4.

Double pneumonia	-	-	-	M. 31. 121.
Diffused bronchitis passing into double pneumonia	-	-	-	M. 29. 60.
Single pneumonia	-	-	-	W. 22. 4.
Single pneumonia	-	-	-	W. 25. 121.

Of rheumatism with endocarditis and pericarditis combined there were:—

Cases 11.—Lungs affected in 8.

Double pleurisy with double hydrothorax	-	-	-	M. 32. 166.
Single pleurisy with hydrothorax	-	-	-	W. 25. 183.
Double pneumonia	-	-	-	W. 22. 110.
Double pneumonia with double pleurisy and double hydrothorax (fatal)	-	-	-	M. 31. 140.
Single pneumonia	-	-	-	M. 26. 44.
Single pneumonia	-	-	-	M. 30. 107.
Pneumonia and diffused bronchitis of one lung (fatal)	-	-	-	M. 25. 29.
Diffused bronchitis of both lungs (fatal)	-	-	-	M. 25. 69.

I know not how I can give you a better notion of what these complications really are, and how they present themselves one after another to clinical observation, and the awful amount of disease that results, than by describing, with some detail, certain cases of acute rheumatism in which they existed, and commenting upon their particulars as I go along.

William Buekley, a stout well-formed man of forty years of age, was admitted into St. Bartholomew's October 27. 1836. He was suffering acute rheumatism, and complained of pain, chiefly in his right shoulder and his right wrist, which was very red and much swollen. His skin was very hot, his pulse ninety-six and hard and full, his urine scanty and high-coloured, his tongue covered with a thick moist white fur. He was thirsty, without appetite, and sleepless at night. But withal his countenance was good and quite free from anxiety; in short, it was the countenance of health. He had no internal pain whatever, and the sounds of his heart were perfectly natural.

Now this man was habitually healthy, and had never suffered acute rheumatism in his life. His present attack was from accidental exposure to cold, ten days ago, which was followed by chilliness, heat, perspiration, and pain. The pain began in his feet, passed to his aneles, and had already visited all his joints in their turn, and been accompanied with heat and swelling in all.

This case surely promised well, and I was content to treat it with ten grains of Dover's powder night and morning, and to interpose the use of active purgatives.

For three entire days, and four entire nights, his existing symptoms remained nearly the same, and no new symptoms were superadded.

On the fourth day the pain and swelling had shifted from the right wrist and shoulder to the left wrist and shoulder. And now, in applying the stethoscope to the region of the heart, a distinct exocardial murmur was heard. It was chiefly at the basis, and lost somewhat of its intensity as the instrument was moved towards the apex.

Here was unquestionable pericarditis. And let me remark that auscultation was regularly made of this man's chest day by day, and was just as much a matter of course (the case being one of acute rheumatism) as feeling his pulse, or looking at his tongue. The disease was not sought after from the patient's drawing attention to the part by any complaint of pain or unusual sensation, or palpitation. And when the disease was found, the patient, in answer to all our questioning, still declared himself quite unconscious of any thing amiss within his chest. It was the fourteenth day of the rheumatism that the pericarditis was first discovered, and it was

most probably discovered as soon as it began to exist. But to proceed with the case:—

The patient being a strong man, and his pulse being now more full and hard, and his fever more fully developed, and a vital organ inflamed, was bled by venæsection to twenty ounces, and ordered three grains of calomel, and a quarter of a grain of opium, every three hours.

The next day the general symptoms being the same, and the exocardial murmur unaltered, he was bled again by venæsection to eight ounces, and four ounces more were taken by cupping from the præordial region, and the calomel and opium were still continued.

The next day he had more power of moving his limbs, and the to-and-fro sound was thought to be a little less distinct.

The next day the fever continuing fully developed, and the pulse full and hard, and the murmur as distinct as ever, and the calomel having been now taken every third hour for four days and four nights, and having not yet produced the least sensible factor of the breath or salivation, ten ounces more of blood were taken from the arm.

And now for the four following days there was a great fluctuation of all the symptoms. The pain in the joints abated and returned, and then abated again. The pulse became less full and hard, but more frequent. He slept well one night, and ill another. The heart gave out a

confusion of sounds which was indescribable. It was doubtful whether salivation was not coming on. Accordingly, for these four days, all active treatment was suspended, while the course and tendency of the symptoms were carefully watched.

The four days ended, it was evident that a new disease had arisen, and been added to that which already existed. The endocardium was inflamed, as well as the pericardium. It was the rise of the auscultatory signs proper to the new disease that had produced the confusion of sounds. But now the sound of endocarditis was more evident than that of pericarditis. The single systolic bellows-murmur was very distinct, while the to and fro sound had degenerated into a mere crumpling. Probably the pericardium was beginning to adhere.

The general state of the patient betokened great debility and distress of the nervous system. The pulse was 120, and small and soft. But this remarkable circumstance deserves to be especially noticed, that although the pericarditis had been going on ten days, and endocarditis was now superadded to it, the patient had not been all along, and was not even now, conscious of the least pain in the region of the heart. The first symptom directly referrible to the heart within his own consciousness, had arisen during the last two days. This was a palpitation whenever he turned quickly round in bed.



And now the treatment was resumed by the application of eight leeches to the præcordial region, and the same dose of calomel and opium at the same intervals as before ; for the suspicion of salivation was fallacious.

During the five following days, the symptoms continued to fluctuate more and more. The constitutional sympathy was transferred more and more from the vascular system to the nervous system. The to and fro sound entirely ceased, and the single systolic bellows-murmur alone remained.

And now it was evident that another new disease had arisen. At the lower part of the left lung a minute crackling had taken place of the respiratory murmur, which too surely denoted pneumonia.

Again, suspected ptyalism admonished us to suspend the use of calomel; and again it was resumed, when no ptyalism was apparent. Still there was no præcordial pain. But still the pain and the swelling returned, and receded capriciously to and from the knees and the wrists.

In three days more the symptoms were all concentrated in the nervous system, and all the treatment consisted in the administration of opium and ammonia. The pulse was very frequent and very feeble; the tongue trembled; the hands trembled; there were frequent perspirations; there was neither endocardial nor exocardial

murmur, or any other definite sounds of an unnatural kind proceeding from the heart. Probably it could not contract with force enough to produce them. There was a mere roughness accompanying its systole.

Still life continued seven days longer. In the meantime the pulse rose to 140, and became more and more feeble; the perspirations were more profuse, and almost continual; the tremblings of the limbs never ceased, and at length became convulsive spasms. His mind wandered all day, except when, his attention being strongly roused, he was brought for a short time to himself. At night he was sleepless, singing, or muttering, or vociferous. Opium, however, procured him sleep during one entire night. He woke refreshed, was rational for a short time, confessed he was free from pain, and then relapsed into delirium. His evacuations passed involuntarily. As his sweats became more copious, his tongue became drier.

At length he seemed made quiet by exhaustion. His last two nights were very tranquil. On the 26th of November he died. It was the fortieth day from the commencement of the rheumatism, the twenty-sixth day from the commencement of the pericarditis, the eighteenth or nineteenth from the commencement of the endocarditis, and the fifteenth or sixteenth from the commencement of the pneumonia.

The exoeardial murmur was distinctly audible only seven days, and in nine days it was entirely gone. Hence we inferred that the pericardium was adherent to the heart. The endoeardial murmur ceased as such in eight days, from the heart having not force of contraction enough to produce it, and so it degenerated into a mere roughness. The erepitation, which denoted the lungs to be inflamed, being once heard at the back, was never afterwards sought after; for it would have been at peril of the patient's life to have raised him in bed, his debility had become so great.

There is an omission of one important circumstance in the record of this case. I find no notice of any dulness to percussion in the præcordial region. In truth, I was not at that time (1836) fully aware of the important intimations derived from percussion in periearditis.

Now, why was not this man cured? When he came into the hospital his complaint was simply rheumatism, and each of its perilous adjuncts arose one by one under our observation, and was treated vigorously and without delay. The man's constitution, too, was good, and capable of bearing well the remedies needed to arrest the progress of inflammation. Yet he died. Did he die because the *great* remedy failed of its specific effect,—because the mercury failed to salivate? The appearances on dissection appertaining to the

heart will be found elsewhere.\* The following was the state of the lungs. Both of them were slightly emphysematous, and neither of them collapsed upon removal of the sternum. The right was crepitous throughout, and loaded throughout with frothy sanguineous fluid; and the surface of every bronchus, as far as it could be traced, was loaded with blood. Every portion of this lung was buoyant in water. Of the left lung the entire lower lobe was hepatized, and sank in water while the upper lobe presented the same pathological conditions as the other lung.

To me this case, as it passed under my observation, was full of interest and instruction, and of painful disappointment. (M. V. 25. p. 29.)

Catherine Sullivan, aged 22, married, a picture of abject want and wretchedness, was admitted into St. Bartholomew's, May 4. 1837. Her skin was hot and perspiring, her pulse 120, and full, but without power, and occasionally faltering in its beats. All her large joints swelled and painful; her breathing now quick and hurried, now pausing and sighing. She complained, moreover, of pain in the præcordial region, and palpitation. She coughed often, but did not expectorate, and in coughing she increased the pain at her heart.

Auscultation found the exocardial murmur most distinct at the basis of the heart, and ex-

\* Vide p. 153.



tending thence to the cartilage of the fifth rib. It found, also, the systolic endocardial murmur, which was, also, loudest at the basis, and gradually became less loud towards the apex.

Such was her actual state. And this was her history. She had lived in vice, and misery, and want. Ten days ago she had begun to suffer pain and swelling of the joints with fever. From the first she had been sensible of indefinite pain and uneasiness of the chest, which two days ago, by gradual increase, had reached their present form and aggravation.

It is hardly possible to conceive a case more unpromising than this. Here were the worst diseases in the worst constitution. They had been in progress probably for more than a week. They had probably reached their extremity two days. The pericarditis and endocarditis and the rheumatism had been hitherto untreated by any remedy. All still remained to be done that could be done.

She was at once cupped to ten ounces between the left scapula and spinal column, and ordered to take two grains of calomel and a quarter of a grain of opium every three hours.

The next day she was found to have passed a quiet night, though without sleep. But her pulse had risen to 140, and was certainly feebler. The præcordial pain had increased, and the palpitation and dyspnoea were undiminished. The same



sounds were heard in the region of the heart. She had had three or four bilious motions.

Thus the change since yesterday was surely for the worse. The greater weakness and greater frequency of the pulse denoted that a collapse of the nervous system was at hand. But one symptom there was, which, among all others that were bad, still furnished me a ground of hope. The tongue was covered with a white fur, and was *very moist*. This seemed to promise a speedy salivation.

She was again cupped between the left scapula and the vertebral column to ten ounces, and the calomel and opium were continued as before.

The next day, the endocardial and the exocardial murmurs being the same, and the pulse being just as feeble and frequent (140) as it was, the cough, and palpitation, and præcordial pain, and pain in the joints were all diminished. The gums were slightly sore.

On the next day the pulse and all the general and local symptoms were the same, while the salivation was on the increase. But on the next the countenance manifestly less anxious, and the pulse reduced for the first time to 120, bespoke some favourable change in the essential conditions of the disease, although the auscultatory signs were yet unaltered. But it was deemed right still to give the calomel and opium every four hours, for the salivation was not yet profuse.

It was now the 8th of May, the fourth day since

the patient's admission. There was a remission of all those symptoms which denote progressive disease, and an apparent possibility of saving her life. This remission, however, only lasted two days. For on the 10th of May the pulse again rose to 140; the dyspnœa returned with pain in every part of the chest, as much on one side as the other, and before as behind. Auscultation found large and small crepitation diffused through both lungs. There was double bronchitis and double pneumonia. The weakness was now so extreme, that no form of bleeding could be thought of. A large blister was applied to the sternum, and the calomel and opium continued.

The next day the dyspnœa had become an indescribable anguish and struggle for breath. On the next the same distress continued, when auscultation found the right side of the chest entirely dull to percussion, and no air entering into any part of the right lung, except at a small space opposite the scapula, where there was the loud puffing of bronchial respiration and the bronchial voice, which amounted to pectoriloquy. I suspect the right pleura had become suddenly filled with fluid. The left lung still admitted air everywhere, but everywhere with a crepitation. As to the heart, the endocardial murmur had entirely ceased, and the exocardial murmur remained alone.

A short description will suffice for the four fol-

lowing days. Salivation had run on to an excessive degree, and produced its worst distress. The fauces swelled, the tongue became too large for the mouth, and a stream of saliva was continually running from it. None but the erect posture could be borne night or day. The respirations were never less than 50 in a minute, or the pulse than 135. She was speechless, and when inquiry was made of her pains, she pointed to the epigastrium and right hypochondrium as their seat.

In this state of things there was no room for further treatment. One great remedy, the mercury, though it was no longer given, was still in constant and unrestrained operation. In truth neither food nor medicine could be administered during these four days. Neither was it possible to make any effectual examination of the chest from fear of killing her if we disturbed the only posture in which she could breathe.

On the 15th of May, being still alive, a large blister was applied to the seat of pain, which she indicated in the epigastrium. On the 16th she gave signs that her pain was relieved; her pulse had fallen to 120; her countenance had lost some of its distress; the breathing was less difficult, and she could lie on her back.

The next three days and nights were a period of comfort. No pain was felt. The respiration was easy and much less frequent, and there was

hardly any cough. The pulse had come down to 120. The salivation and all its attendant distresses were greatly lessened. Yet there remained still some pain in the limbs. The endocardial murmur again accompanied the systole of the heart, but the to and fro sound had entirely ceased. I could not bring myself to disturb her tranquillity by raising her in bed for the purpose of learning, by auscultation, the state of the lungs.

Now there was a fair hope that the work of reparation had begun, and that it would proceed without further interruption; but on the 19th it was found that such pain in the chest and dyspnœa had come on during the night as to prevent her lying or sleeping. She was still in the erect posture, and the pain and dyspnœa continued, and the pulse had run up to 140. The whole of the right side was as absolutely dull everywhere as it was when the last auscultation was made. I ventured to draw a few ounces of blood by cupping glasses applied to the back below the right scapula; and ordered two grains of calomel and a quarter of a grain of opium to be taken that night and the next morning.

The next day the dyspnœa was greatly diminished, and the pain within the chest was entirely gone. From this time convalescence proceeded slowly, but uninterruptedly. On the 30th the left lung had altogether recovered its

healthy condition, and a feeble respiratory murmur was heard in every part of the right.

The patient remained in the hospital several weeks longer, gradually recovering all the general conditions of health. She left the hospital with the right lung restored almost to an equal capacity of breathing with the left, and the endocardial murmur still accompanying the systole of the heart, and the exocardial murmur gone. (W. 22. p. 110.)



## LECTURE X.

THE TREATMENT OF ACUTE RHEUMATISM CONSIDERED, PREPARATORY TO THE TREATMENT OF ITS ACCOMPANIMENTS, ENDOCARDITIS AND PERICARDITIS.—ACUTE RHEUMATISM SUCCESSFULLY TREATED, UPON DIFFERENT AND EVEN OPPOSITE INDICATIONS, AND BY DIFFERENT AND EVEN OPPOSITE REMEDIES.—HOW THIS MAY BE WITHOUT DISPARAGEMENT OF MEDICINE AS A SCIENCE.—THE TREATMENT OF ALMOST ALL CURABLE DISEASES NARROWED TO THE CHOICE OF A FEW INDICATIONS AND A FEW REMEDIES.—WHAT THE LOWEST AND WHAT THE HIGHEST OFFICE OF THE PHYSICIAN.—THE HIGHEST ENGAGED IN THE TREATMENT OF ACUTE RHEUMATISM AND ITS INCIDENTS.—THE GROUNDWORK OF RATIONAL PRACTICE IS TO UNDERSTAND THE VALUE OF SINGLE INDICATIONS, AND THE POWER OF SINGLE REMEDIES.—TREATMENT OF ACUTE RHEUMATISM UPON INDICATIONS BELONGING SOLELY TO THE VASCULAR SYSTEM, AND SOLELY BY BLEEDING.—UPON INDICATIONS BELONGING SOLELY TO THE NERVOUS SYSTEM, AND SOLELY BY OPIUM.—UPON INDICATIONS BELONGING SOLELY TO ABDOMINAL VISCERA, AND SOLELY BY CALOMEL AND PURGATIVES.

ALL our inquiries, as far as they have gone, into the subjects of endocarditis and pericarditis may be very interesting, and all our speculations may be just and true. But however interesting, just, and true, they must not terminate here. Our patients at least have a farther concern with these

diseases, and so should we—a concern, namely, how they are to be treated.

Now, endocarditis and pericarditis are the same things in essence. They are both inflammations. And endocarditis and pericarditis are annexed to the same pathological condition of the constitution at large. They both belong to acute rheumatism. Moreover, they often occur together in the same subjects. Thus the treatment of both is by the same remedies, and may be spoken of together.

But the treatment of the general pathological condition out of which they both spring, first claims our attention. For the right or wrong management of the rheumatism may have a share in determining whether these, which for the present we must be content to call its incidents, shall or shall not take place at all. And further, if they *do* take place, what shall be their character, and what their degrees of severity.

Acute rheumatism has experienced strange things at the hands of medical men. No disease has been treated by such various and opposite methods. Venæsection has wrought its cure, and so has opium, and so has calomel, and so has colchicum, and so have drastic purgatives. I speak of these remedies in the sense which medical men imply when they talk (as they sometimes do) of this, that, or the other thing being their “sheet anchor;” meaning that they rest upon it alone for

the cure of the rheumatism, and employ other remedies either not at all, or for very subordinate purposes. And, indeed, I bear my testimony to the success of each of these different remedies, so far as that, under the use of each, I have seen patients *get* well.

At the first view all this looks very strange. The cure, or seeming cure, of the same disease by different remedies, even by remedies which in their mode of operation have nothing in common, appears like luck or accident. At the first view it may shake one's faith in physic a little, and may a little excuse the pleasantry of some who choose to hint, that Nature is our best friend after all ; for that, do what we will, she brings things to a prosperous issue in spite of our blind interference.

But without disparaging the part that Nature plays, I here see no fair subject of ridicule, and no fair reason for distrust of methods of rational treatment. The first maxim of all rational practice is, that nature is supreme ; the next, that nature is obsequious. The end, whether bad or good, death or recovery, and every step and stage conducive to it, are the unquestionable work of Nature. But Nature, in all her powers and operations, allows herself to be led, directed, and controlled. And to lead, direct, or control for purposes of good, this is the business of the physician. But how to do it best, he has to exercise

a choice of modes and means in every case, which, though never exempt from the possibility of error, becomes less fallible by the teaching of experience.

This choice leads, and always will lead, to diversity of practice, which in no way disparages, but rather tends to enlarge and to enrich the resources of our art.

It is not possible that the treatment of diseases shall be ever set at rest by the consent of physicians, or that fixed and uniform plans and remedies shall ever be adopted in cases bearing the same nosological name and character. At least it cannot be until each disease has its own specific antidote, or until each has disclosed some seminal principle from which it springs, and shown where it is, and what it is: some principle, too, it must be which is within reach, and which is destructible and easily destroyed.

But we know very little about the seminal principle of diseases, and that little serves to show that no sooner does it enter the body (as in the case of contagion), than it is gone at once beyond our reach. It germinates in secret. It spreads itself abroad in secret. And when, at length, it excites various organs and systems to extraordinary modes of action and suffering, then, and not sooner, begins our knowledge of a present disease, and our power of interfering with it. In truth, these modes of action and suffering are *to*

*us the* disease. They are, moreover, our only objects of medical treatment.

In cases of fever from contagion, in cases of inflammation, which is independent of external violence—spontaneous inflammation, as it is called—much must have been going on covertly within the body before those modes of action and suffering arise, which are plainly cognisable by us, and which we can interfere with. But coming in with our knowledge and our treatment when we do, and late as we may seem to do, we are nevertheless soon enough with both to perform the proper work of physicians, and to withhold the disease from terminating in death or disorganisation. For it is by these actions and sufferings—it is by the actions and motions of bloodvessels, or by the feelings and susceptibilities of nerves, or by the special functions of particular organs, such as the stomach and bowels, the liver and kidneys, being extraordinarily exalted or extraordinarily depressed, or variously disturbed and perverted, it is by one or by several of these that diseases, of whose seminal principle we have no knowledge, declare their existence, and denote their progress and tendency; and it is by and through the same that they are brought into a capacity of treatment and of cure. The vascular system as a whole, and the nervous system as a whole, and the particular organs indicated, are in an especial manner within the reach and power of medicine. It is



by remedies operating through some of them that nearly all curable diseases are in effect cured.

But our present business is only with the treatment of acute rheumatism. Yet these general remarks, introductory to it, will not, I trust, be thrown away. For the treatment of acute rheumatism, above all other diseases which can be named, is a thing to put the physician and medicine itself to the trial of what they can really do. Here are no specifics at hand. All proceeds upon rational calculations; upon the right choice of purposes to be fulfilled, and the right choice of means to fulfil them. Acute rheumatism is often such in itself and such in its appalling incidents, as to need from time to time that medicine should put forth the full compass of all its powers. Every organ or system of organs which, either directly or indirectly, can receive the impression of remedies, are from time to time called to bear all that they can possibly endure; and it is often only when the powers of medicine are pressed even to the verge of destroying life, that life is saved.

If the treatment of acute rheumatism ever come to this, it is right to know what we are about when we undertake to treat it.

It has been said that, in the treatment of acute rheumatism, one trusts entirely to venæsection and cures it, another to opium and cures it, and another to drastic purgatives and cures it. Here,

among several indications which offer themselves to his choice, the physician takes a single one, and makes it the sole mark and scope of his practice, trusting that, when he has effectually attained it, the complex actions and sufferings which constitute the disease will be brought to an end.

Thus he takes the high vascular action of acute rheumatism, and sees the whole disease represented in it, and is solely intent upon subduing it by venæsection, expecting that, as he pulls down the circulation, the fever, the nervous disquietude, and the pain, and the swelling will all cease, and the various secreting organs of the body will resume their natural functions, and that thus the actions of health will gradually supersede the actions of disease. Or he takes the nervous disquietude and the pain of acute rheumatism as the representative of the entire disease, and deals with it accordingly, being solely intent upon moderating them with opium, and expecting that, as they subside, the high vascular action and the fever and the swelling will subside along with them, and that the secretions will return to their healthy measure and kind. Or he takes the state of the several secretions, their deficient quantity, and their unhealthy quality, as the representative of the entire disease, and so addresses his treatment to those organs whose secretory functions are more immediately within

the reach of medicine, to the stomach and bowels and liver, and he gives large and repeated doses of calomel, and follows them with large and repeated doses of purgative medicine. This he does, and this is all that he does; and having done it effectually for a few days, and obtained very large and bilious evacuations, he expects that the fever, and high vascular action, and nervous disquietude, and pain and swelling will all cease, and the patient will be well.

Let me repeat my testimony to the success of this practice in acute rheumatism; the practice, namely, of choosing some single indication, and steadily pursuing it to its fulfilment. It is a very rational practice. It is founded upon experience, and it compasses its end by very simple means; and the manner of its successful operation may be well conceived, if it cannot be entirely explained, in the present state of our knowledge. Disease is a series of new and extraordinary actions. Each link in the series is essential to the integrity of the whole. Let one link be fairly broken, and this integrity is spoiled, and there is an end of the disease; and then the constitution is left to resume its old and accustomed actions, which are the actions of health.

But, you may ask, Is the treatment of acute rheumatism really so plain and simple an affair in all cases? Is there nothing else to be done, but out of several purposes (or indications of treat-

ment, as we call them) to choose judiciously some single one, and pursue it resolutely and effectually by the simplest means? And is this the practice to which the cure of the disease may be safely trusted in all cases? Certainly not — certainly in a small proportion of them only.

But it is not without reason that I have dwelt upon this practice of single indications and single remedies. For, though capable of being strictly followed in a few cases only, it contains a principle of large application, which helps and furthers the treatment of all cases of this disease, and of many diseases besides.

The lowest office of medicine is to minister to mere ailments; and this is most effectually done by telling people what in their ordinary mode of living is injurious, and warning them against it. But inasmuch as injurious things are commonly very pleasant things, people are reluctant to leave them off at our mere bidding. Hence in this, which is their humblest province, small credit upon the whole has been gained by the best physicians. The advice they have to give is much too simple for the world to accept upon the credit and character of well instructed and honest men. It needs to be enforced, not by the art which they *do* practise, but by the mystery which they *do not*. For no sooner does the same recommendation of abstinence from what is injurious gain the help which it needs from the



mystery of homœopathy (a mere name, importing the discovery of something unintelligible, and importing it for the popular enlightenment in Greece) than compliance becomes easy, cures multiply, fame vociferates, the glory is great, and great too is the emolument.

But the highest office of medicine is to minister to diseases, which, by themselves or by their incidents, go directly and rapidly to the destruction of life. And this is not to be done by begging people to be reasonable and abstain from what is wrong, and cheating and cajoling them into compliance. But it is a business for wise and cautious men alone to meddle with. The powers of art must be brought to overrule the operations of nature by force. To know these powers and how to wield them to such a purpose is an affair beyond all trick and all skill of practising upon the fancies of mankind. It can only proceed from a faithful and candid search after truth by each of us for himself according to his opportunities, and from a ready communication of what we believe to be the truth by all of us among one another, and from a comparison of their experiences and conclusions among the best minds.

Now the highest office of medicine, in the sense explained, is engaged in the treatment of acute rheumatism and its incidents. At every step of its treatment, principles are involved



which may be transferred to the treatment of almost all diseases in which medicine plays an active and indispensable part. While, therefore, I proceed to tell you of the management of acute rheumatism, you must consider me as illustrating the general power of remedies by this example, and so be prepared for any general remarks I may let in as I go along.

Well, then, I am persuaded that when the physician is called upon to perform great things, even to arrest destructive disease, and to save life, his skill in wielding the implements of his art rests mainly upon the right understanding of simple and single indications, and of the remedies which have power to fulfil them. To know that, in any cases of acute rheumatism, the cure of the entire disease is accessible through the blood-vessels alone, by subduing their too forcible action; or accessible through the nervous system alone, by moderating its disquietude, and abating pain; or accessible through the stomach and bowels and liver alone, by stimulating them to a very large amount of secretions; and to know, moreover, that in any cases venæsection alone will fulfil the first purpose, or opium alone will fulfil the second, or calomel and aperients alone will fulfil the third; to know all this is the best preliminary step towards enabling us to deal safely and successfully with other and more numerous cases, which need a more complex method

of treatment, and require that all these important purposes be pursued simultaneously, and all these great remedies be made confederate for the cure of the disease.

As to the first of these great remedies, and the indications for employing it, I know no disease in which febrile heat is greater, and the pulse harder, fuller, and more forcible, than in acute rheumatism. What, then, are the signals for copious blood-letting, if these are not? And together with heat so great, and the pulse so hard, so full, and so forcible, unquestionable inflammation is present in various parts of the body: and what can further demand and justify copious blood-letting, if this does not?

But let us pause and consider awhile. When inflammation is unquestionable, and when it has newly arisen, and with it fever and excess of vascular action, the amount of this fever and vascular action becomes the measure of its treatment. And it becomes so, because it is in truth the measure of the inflammation itself, *i. e.* the measure of its force, and the rate of its progress towards whatever *evil event it may tend, either according to its own nature, or according to the nature of the parts it occupies.* In most parts of the body such an amount of fever and vascular action, as usually accompanies acute rheumatism, would denote inflammation tending rapidly to its worst event—to some kind of destructive disorganisation.

It would so in the brain; it would so in the lungs; it would so in the liver. Therefore, in inflammation of the brain, the lungs, or the liver, such fever and such vascular action would call for any quantity of blood-letting that might be needed to subdue them. For their abatement alone would show the inflammation abated, and its destructive progress arrested.

But in rheumatism the inflammation (I mean the external characteristic inflammation), either from its own nature, or from the structures it occupies, tends to no such destructive event. It has its primary seat (there is reason to believe) in fibrous structures, and the swellings which accompany it are produced by serous effusion, partly into neighbouring cellular texture, and partly into the synovial membranes of bursæ and joints. And in the vast majority of cases, however severe be the disease, and however long it may last, the local mischief stops with these effusions, and the structures engaged, fibrous, cellular, and serous, undergo perfect reparation, and the joints are eventually uninjured.

In acute rheumatism, therefore, the excessive heat, and the full, hard, and forcible pulse need not hurry us into a determination to bleed to any amount that may be requisite to subdue them, with the ultimate purpose of withholding the inflammation from a destructive event. But the aim and intention of the remedy are rather re-

spective to the disease *as a whole*, than to the inflammation attending it. It would be otherwise if the liver, the lungs, or the brain were inflamed, and not the joints.

With respect to the disease as a whole, then, let us consider the uses of this remedy.

I have seen people enormously bled in acute rheumatism, and their entire disease swept away at once, and health restored rapidly. And the practice which will do this, is it not a splendid and a tempting practice?

Again; I have seen people enormously bled in acute rheumatism, and their disease swept away at once; but they have forthwith gone raving mad. And a practice which will do this, is it not a hazardous practice?

And again; I have seen people enormously bled in acute rheumatism, and no single pain has been mitigated; but the disease has continued for an unusually long time in its acute, and then has degenerated into its chronic, form. And a practice that has this issue, is it not a doubtful practice?

These facts are instructive. They warn us to beware of large bleeding in acute rheumatism. Yet the immediate indications for it *now*, when it would (I believe) be erroneously prescribed, are the same upon which it would be entirely justifiable in some other diseases, viz. the extreme heat and the extreme fulness and hardness of



the pulse. But then in those other diseases, all hazards sink into insignificance compared with the hazard of progressive inflammation in a vital organ. Large bloodletting, however, has hazards of its own great enough not to be incurred except in exchange for those which are far greater.

Now, one chief hazard of large bleeding is from the shock it is apt to communicate to the nervous system; and it is this which we should especially seek to avoid, when we employ venæsection in acute rheumatism. Venæsection is often needed; needed for what no other remedy can perform towards the cure, and therefore not to be omitted. It is needed especially to abate high vascular action. But no such thing must be thought of as bleeding and bleeding, until the large pulse becomes small, and the hard pulse soft. For, to bring about this, blood must be let flow to a terrific amount. Fulness and hardness of pulse are indeed express characteristics of acute rheumatism, rising to superlative degrees, and enduring pertinaciously, and resisting stubbornly the power of remedies to pull them down. To abolish them at once or speedily in the severer cases, is hardly possible by any counteracting means which medicine can safely employ. They are above a match for them all, and they *will* endure their time. They are above a match even for venæsection, unless it be pushed with a desperate hand, careless and reckless of new dangers to life.



Upon the whole, then, the practice which proposes to compass the cure of acute rheumatism at whatever cost of blood may be needed to fulfil the indication of absolutely subduing the force of vascular action, is a very uncertain and a very dangerous practice, although success has undeniably attended it in some instances.

Still venæsection is among the remedies of acute rheumatism, not needful in all cases, but expedient in many. It is expedient to abate vascular action when it is excessive, and when the patient is robust and young, and the disease has arisen accidentally in a healthy constitution. And these, indeed, are its most common conditions. Acute rheumatism is most frequently found in the young, the robust, and the previously healthy. But however the severity of the disease, and the age and constitution of the subject, may invite the remedy, this caution especially must be observed in its use; take care that in abating vascular action by venæsection, you do not communicate a shock to the nervous system. If you do, you are likely to disturb the just tenor of the disease, and then some untoward circumstance, which is quite foreign to it, may arise, and some of its worst events may ensue.

But in the young, the robust, and the previously healthy, where vascular action is not excessive, and in the old, the feeble, and previously valetudinary, even where it is, venæsection is best

omitted. There are other remedies, which, without the help of venæsection, may be trusted for its safe and effectual cure.

Summarily then I would venture to say of venæsection, employed under the most suitable conditions, and in the most suitable measure, that it is to be trusted, rather as preparatory and auxiliary to other remedies, than for its own exclusive remedial power in acute rheumatism. It very often renders the disease more curable by other means; but it seldom cures the disease itself.

But it has been said that the cure of acute rheumatism is accessible through the nervous system alone, and by means of opium.

No disease can be mentioned of which pain is a more prominent and abiding characteristic; hardly any in which it is more severe and more extensive, and occupies more situations at a time. It is, moreover, the sort of pain which rouses and excites, and however long it may continue, even for several weeks perhaps, it still rouses and excites to the last.

This is a circumstance which deserves our notice. The pain which is annexed to inflammation of internal and vital organs, however it may at first rouse and excite, soon begins to depress, then to exhaust, and then to overwhelm; and as the disease proceeds, and as the vital powers fail, the nervous system sinks into an absolute incapacity of feeling pain at all. But

the pain which belongs to the inflammation of external parts and parts not vital, continues much longer to rouse and to excite, because life itself, or the springs of life, continue much longer unassailed. Thus, in acute rheumatism, the nervous system and the vascular system, the great sources of action and feeling, being unharmed, feel and resent the disease purely as a stimulus. As long as the inflammation is of the fibrous, cellular, and serous structures in the neighbourhood of joints, and of them only, the brain and the nerves, the heart and the bloodvessels, and all that *feels* and all that *acts* within the body, have their susceptibilities and their movements raised, quickened, and invigorated. All within the man is (as it were) doubly alive; and every thing that hurts is doubly felt, and doubly resented.

If, then, such be the pain, and such the long and undiminished capacity of suffering in acute rheumatism, well might physicians, in their treatment of it, look to such remedies as abate pain, and exercise a sedative influence upon the nervous system. They have, indeed, looked especially to opium. And opium, prescribed with these intentions, has encountered the disease single-handed and successfully. But *how* prescribed, and in *what* measure?

A grain of opium given to a man in health and at ease, would continue to be felt for twenty-

four hours; not so when it is given to a man in constant and severe pain. The same quantity would now be felt little or not at all, or for a much shorter time.

The nervous system may become wild with suffering, and then it is not to be soothed and coaxed into quietude, but to be subdued. It is with the nervous system exasperated by physical, as it is with the mind exasperated by moral, pain. The maniac cannot be pacified by persuasion; he must be held down by some power which he cannot resist; and even then he is not absolutely still. The victim of cancerous disease must be overpowered and stupified with opium, and even then he is not altogether insensible to his pain.

The pain of acute rheumatism will hardly bear to be compared with that of cancer. But it is a very sharp and constant pain, and severely aggravated by the least movement and the least touch. The dose of opium must be large, and pretty often repeated, which is to reach it and lessen it. In the severer cases, and when the whole treatment of the disease is left to the sole remedial power of opium, the measure and frequency of its dose must be enough to *subdue*, if it is to have a fair chance of tranquillising.

When first, and for a few years after, I became physician to an hospital, opium was my remedy in all cases of acute rheumatism, excepting such as presented some special circumstance to forbid its



use, or to require a different treatment. My single purpose was to abate pain, and to quiet the nervous system. The dose I employed varied from two grains to five or six in twenty-four hours. I began with one grain every twelve hours. Then, as the patient seemed to bear it or to need it, I gave a grain every eight hours, then every six, and then as often as every four. There were many cases for which a grain every twelve hours, or two grains in the twenty-four, were quite enough. And there were few for which a grain every four hours, or as much as six grains in the twenty-four, were needed. The majority, however, required a grain every eight or every six hours, or three or four grains in the twenty-four. In the mean time, while I thus employed opium *immediately* to abate pain and quiet the nervous system, and *ultimately* to cure the disease, I had no other care except to keep the bowels from being bound, but not to purge them.

And now, perhaps, you may be disposed to suspect, after all, that this practice had nothing positively remedial in it; that the progress and duration of the disease were in nowise affected by it; and that only its pains were thus rendered more tolerable, while it wore itself out by a spontaneous and unassisted reparation.

But I am very far from thinking so. It is true that I cannot produce a certain number of cases treated by no remedy at all, and compare



them in their results with a certain number of cases treated by opium. I do not know that I ever saw a case of acute rheumatism left entirely to itself. But I have seen many cases, if not altogether abandoned to nature, very little assisted by art. When I was a student, acute rheumatism in the hospitals of London was commonly treated thus:—A dose of *liq. ammonia acetatis* was given thrice in the twenty-four hours, and a moderate opiate at night. Such treatment cannot be called nugatory, or tantamount to no treatment at all. It might even, where pain and vascular action were small, have been quite treatment enough, and have justly had the credit of the cure. But in the majority of cases it could have done very little. In the majority there was more of action and suffering than it could have power to counteract. The means themselves had no unsuitableness to their end; but there was a short-coming in the way of using them. The means had plenty of remedial force in store; but that force was not brought out, as it might have been, by the time and measure of their application, and so made more a match for the force of the disease, and more available for its cure. Accordingly, by this treatment (to speak of its results as favourably as possible), the acute rheumatism was seldom brought to a close in less than six weeks. It took full six weeks, upon an average, to get the

patient out of bed and fairly upon his legs again. And then he had still his strength to regain that he might be fit for work. Whereas, under the treatment by opium (given not merely in a moderate dose and at night only with the hope of procuring sleep, but at more frequent intervals and in quantity proportionate to the amount of pain and nervous disquietude it had to subdue or to mitigate) the disease has commonly ended, and convalescence has fairly begun in half the time.

Such, then, is my experience of the curative impression, which may be conveyed to the entire disease solely through the nervous system, and solely by means of opium. And let me add that, considering what acute rheumatism is in the majority of cases, and what it needs, and what it will bear, I regard the indication found in the nervous system to be upon the whole a safer and better guide for its treatment than that found in the vascular, and opium upon the whole to be a safer and better remedy than venæsection; if we are to follow one of the two indications, and to use one of the two remedies only.

But recollect, I am not recommending that acute rheumatism be treated exclusively, either according to the one or the other of these indications; or exclusively, either by the one or the other of these remedies. I am only now showing the value of each indication, and the power of each remedy separately, in order that you may

better understand and appreciate other more complex methods of treatment in which both indications are followed, and both remedies are employed concurrently.

The third plan remains to be considered, which, passing by the vascular system and the nervous system and all indications of treatment to be found in either, fixes upon the liver and the abdominal viscera, and seeks to compass the cure of the entire disease by remedies addressed solely to them.

The immediate object of this practice is to obtain from these organs a vast augmentation in the amount of fluids which they secrete, and to evacuate it outwards. The mode of proceeding is this. Ten grains of calomel are given at night, and a draught of salts and senna on the following morning; and the same are repeated night and morning as long as they are well borne, and continue to produce the effect desired.

The evidence of their being well borne is, that they occasion little or no distress in their operations; and the effect desired is, that they bring away abundance of a dark or deep-coloured bilious colluvies from the bowels. As long as such are the feelings of the patient under their operation, and such their effects, the medicines may still be given, and still are needed.

The evidence of their being ill borne is that they occasion tormina and tenesmus, and scalding

of the rectum; and their effect not to be desired is, that they bring away pure bile, or transparent mucus or blood. When such begin to be the patient's feelings, and such the products of the medicines, this plan of proceeding must be immediately given up.

Now, it is for three or four nights and mornings consecutively that this dose of ealomel and this purgative draught will be well borne in the majority of cases; and the better they are borne, the more likely they are to do good.

But the dose of ealomel need not be exactly ten grains, neither more or less. Finding ten do too little, the next night I have given twenty. Finding ten do too much, the next night I have given five: only I would remark, that the dose of ealomel must be a considerable dose. If your patient will not bear five grains, this method of treatment is not for *him*, and the sooner you back out of it the better.

When the full dose of ealomel and the purgative draught have been given for three or four successive nights and successive mornings, and have been well borne, and have had the effect which we desire upon the liver and the abdominal viscera, it sometimes happens that the same remedies are thenceforth no longer needed, and can no longer be borne in any dose. They have well fulfilled their immediate purpose, and done



all they can do for the ultimate cure. But sometimes they are both needed, and can be borne a little longer, yet in a smaller dose. A grain or two of calomel at night and a moderate aperient in the morning still continue to be required for procuring daily evacuations.

As this plan of treatment works prosperously day after day in its immediate effects, so day after day it gives an earnest of the remedial impression it is exercising upon the whole disease. It abates the fever, it softens the pulse, it reduces the swelling, and it lessens the pain. In short, it subdues the vascular system like a bleeding, and pacifies the nervous system like an opiate; and often in the course of a week the acute rheumatism is gone. In three days there is often a signal mitigation of all the symptoms; and in a week I have often seen patients, who have been carried helpless into the hospital, and shrieking at the least jar or touch or movement of their limbs, risen from their beds, and walking about the ward quite free from pain.\*

Of this plan, often so striking in its operation, and often so satisfactory in its results, I have some further remarks to make. It is called the purgative plan; yet its purpose is achieved by calomel and purgatives conjointly. The pur-

\* The profession owes this practice to Dr. Chambers. Its power and efficacy are displayed in numerous diseases besides rheumatism, and in their most perilous emergencies.



gatives would not answer the end without the calomel; of that I am quite certain: neither would the calomel answer without the purgatives, unless it produced of itself ample evacuations from the bowels. It is probable, in short, that the remedial efficacy of the plan resides essentially in the calomel: in calomel, however, not as *mercury*, but as itself — *calomel*. If the specific effect of mercury — salivation — arise, it is not only beside our purpose, and against our wish, but it begets a serious hindrance to the use of calomel in sufficient quantity for the end in view. Thus the whole plan is frustrated. Having begun one plan of treatment, we are obliged to take up with another. Time is lost, the case is perplexed, the disease is prolonged, and the patient perhaps injured.

This is an accident liable to attend the present plan of treatment. It *will* take place sometimes. It is quite unavoidable. Men bear no mark that I know of denoting their great or their small susceptibility to mercury. Of this we must take our chance, knowing that, be the natural susceptibility what it may, fevers and inflammations seem counteractive of it to the extent sometimes of holding it in check, sometimes of suppressing it, and sometimes of abolishing it altogether.

The curative effect of calomel then being annexed to its operation upon the abdominal viscera, we should seek by all means in our power to determine it thither. For this purpose let it

neither dwell too long within the bowels, nor be too soon hurried through them. Ten grains of calomel being given, should be left ten hours to do its own work alone and undisturbed. Its proper work is to impart a peculiar stimulus to the liver and the intestinal canal, and so to promote a large flow of bile and various secreted fluids into the bowels. Time is required for all this; and the purgative should be delayed until the excrementitious matter is first formed and accumulated, and ready to be brought away.

Now, if in the treatment of acute rheumatism you were to choose one indication and abide by it, and were to trust to one class of remedies, and to it only, you would find more cases that admit of a readier cure by the method now described, than by either of the two former. You would find the aggregate of morbid actions and sufferings, which constitute the disease, more surely reached and counteracted, and more quickly abolished by medicines operating upon the abdominal viscera only, than by those which influence either the blood-vessels only, or the nerves only. You would find in calomel and purgatives a better remedy than either in venæsection or in opium.

There might be occasions and circumstances when the bloodvessels or the nerves would offer the best channels, and venæsection or opium would offer the best means of cure. But the occasions and circumstances most frequently pre-

senting themselves would rather lead you to attempt the cure through the abdominal viscera, and by calomel and purgatives.

Another word upon this plan of treatment, and I have done. It has appeared to me not only to bring the disease to a conclusion in a shorter time, but to prepare the way for a more rapid convalescence than the other methods.

When the cure of such a disease as acute rheumatism is largely promoted, or altogether achieved, by calomel and aperients, it will (I rather think) often turn out upon enquiry, that the patients prior to their attack were in a state of health which needed, or would have been none the worse for, a good purging. Many and many a man have I known, who, having suffered fever of high vascular action, and been successfully treated by remedies addressed day after day exclusively to the bowels, has recovered rapidly, and thenceforth has enjoyed better health than he had known for years before. Now, is this the *critical* event of the fever, or is it the effect of the remedy? I believe the latter. I believe that his fever, severe and perilous as it might be while it lasted, was to *him* after all a happy event; inasmuch as he gained by it that medical discipline which he most needed, and which, but for it, he would not have had the benefit of.

Seeing then that by the use of calomel and purgatives in the manner described, patients have

so soon thrown off their disease, and have so soon resumed the conditions of health, do I therefore finally recommend this last method as the common anchor of your treatment in acute rheumatism, and allow a place for either of the other two only when exceptions call for them? Assuredly not! The best single method, or the best single remedy, is not so good but that it may be made better by the help of other methods and other remedies.

It is true that I have seen the bleeding plan, the opiate plan, and the purgative plan each used alone. I have myself used each alone, and the two latter largely; and I have told you what, according to my belief, is their value absolutely and comparatively.

But there is a plan of treating acute rheumatism which is juster and safer, and applicable to more cases, and more successful than any of them. And that plan is a compound of all three.

This compound method, while it works with all the means which have been recommended, stops short of what is harsh and excessive in their use, and yet compasses with more certainty the successful result.

For I believe, that in the treatment of this disease, and in the same cases, by the judicious use of opium you may spare blood, and by the judicious use of bleeding you may spare opium; that by calomel and purgatives properly administered, you may make bleeding and opium less

needful, and that by bleeding and opium discreetly employed you may leave less to be effected by calomel and purgatives.

Hitherto you have seen how, in the management of acute rheumatism, you may deal with bloodvessels, and with nerves, and with secreting organs separately, and with what effect. Presently you will see how and with what effect you may deal with them simultaneously; and how your different remedies once set a-foot, and pursuing different paths, meet and end in one purpose,—and that purpose the cure.

“As many arrows, loosed several ways,  
Fly to one mark.”



## LECTURE XI.

TREATMENT OF ACUTE RHEUMATISM CONTINUED. — ITS TREATMENT ACCORDING TO MIXED INDICATIONS, AND BY MIXED REMEDIES. — THE BLOOD VESSELS, THE NERVES, AND THE ABDOMINAL VISCERA, BROUGHT SIMULTANEOUSLY UNDER THE REMEDIAL IMPRESSIONS OF BLEEDING, AND OPIUM, AND PURGATIVES. — ADVANTAGES OF THIS TREATMENT. — OBSERVATIONS ON THE USE OF COLCHICUM. — REPRESENTATIONS OF MEDICAL TREATMENT OFTEN FALLACIOUS FROM BEING TOO FAVOURABLE. — COMMONLY DRAWN FROM GOOD CASES ONLY; NOT FROM ALL CASES, GOOD AND BAD. — THE GOOD CASES OF ACUTE RHEUMATISM, OR THOSE FAVOURABLE FOR MEDICAL TREATMENT. — THE BAD CASES, OR THOSE UNFAVOURABLE FOR MEDICAL TREATMENT. — NOTICE OF CASES IN WHICH TREATMENT SUCCEEDS OR FAILS, CONTRARY TO EXPECTATION. — NOTICE OF MEDICINES, WHOSE OPERATION IN THIS DISEASE IS UNQUESTIONABLY REMEDIAL, YET NOT UNDERSTOOD.

THE vascular system, the nervous system, and the abdominal viscera are the channels through which, and venæsection, opium, and calomel with purgatives, are the means by which, acute rheumatism is treated and cured. Treated and cured, it may be, through any one of these channels, and by any one of these means, singly. But it may be *through* more than one of these channels, and *by* more than one of these means, conjointly. Or

it may be *through* all of them, and *by* all of them, together.

The practice which chooses and follows a single indication, and chooses and trusts to a single remedy, is indeed a plain and intelligible, but a harsh and subduing, practice. In cases of ordinary severity, if the entire disease is to be effectually reached and counteracted through the bloodvessels alone, a single venæsection of large amount would be needed, or a venæsection of smaller amount once or twice repeated. If through the nerves alone, four or five grains of opium would be needed in each twenty-four hours. If through the abdominal viscera alone, a large dose of calomel and a draught of senna and salts would be required night and morning for three or four successive days.

Here is much violence done and felt as the price of success. Venæsection, single-handed, to do its work successfully, must strike with the violence that shocks, opium with the violence that oppresses, calomel and purgatives with the violence that hurts and irritates. It cannot be otherwise. If one remedy is to do all, it must be heavily charged and resolutely driven home to its purpose.

But each remedy may be charged with less force, if one be made auxiliary to the other. Bloodvessels, and nerves, and abdominal viscera may be severally spared the shock, the oppression

and the pain, if they are subjected simultaneously to their several remedies. Thus, in cases of ordinary severity, a single moderate venæsection instead of several, or instead of one of large amount; two grains of opium distributed over twenty-four hours instead of four or five grains; moderate doses of calomel followed by purgatives instead of very large doses given and repeated for three or four successive nights and mornings, comprise a treatment powerful enough, and always safe, and generally successful, and not painfully felt. Perhaps it would come pretty near the truth to say, that two-thirds less of blood-letting, two-thirds less of opium, two-thirds less of calomel and purgatives, are needed when they are all made confederate for the cure of acute rheumatism, than when any one of them is employed alone.

In several cases, however, the proportion of the remedies to each other would vary. Sometimes more, sometimes less, blood-letting would be called for, and often none at all. So, too, more or less opium, and more or less of calomel and aperients.

I have remarked of the effect of blood-letting in acute rheumatism, that it belongs to it rather to render the disease more curable by other remedies than to cure it itself. Blood-letting, therefore, properly takes the lead of other remedies in point of time. For if it be necessary, the whole treatment must tarry, and other remedies

come short of the good of which they are capable, until it is performed. On the first view, then, of the patient, the immediate question is—Should he be bled? and if so, to what amount? This question it would be foolish and dangerous to pretend to settle any where but in the wards of the hospital, and with the very patient before you, and your finger upon his pulse. Well! and what then? Why! then, if you judge that there is more force of circulation than calomel and purgatives operating upon the bowels, aided by the soothing effects of opium upon the nervous system, will be able to abate, you may bleed.

This is the best direction I can give. But, be it the best and wisest that can be given, it must be utterly useless nevertheless, except to those who are or shall be constantly busied about the sick. For by this direction, whether to bleed or not to bleed is made to wait upon a judgment which must first be formed upon two points. And these two points nothing but the most constant bedside experience can make sure of. They are these—the exact force of the circulation, in a particular instance, to be ascertained by the pulse; and the probable power of calomel, and purgatives, and opium to reduce it, to be estimated by the ordinary effects of the same remedies.

I do not wish to exaggerate the difficulties of medical practice; neither do I wish to conceal them. I am sure you will never surmount them,



unless you first feel and acknowledge them. And some practical experience is needed even for this.

When from the pulse I have considered venæsection necessary to bring down the circulation, the loss of between twelve and sixteen ounces of blood has generally been enough to answer the purpose in view; and the venæsection has seldom been repeated.

The opium, and calomel, and purgatives I have been accustomed to give in combination thus: — With the calomel administered at night, according to its quantity, I have united more or less of opium. To ten grains of calomel I have added one grain of opium; or to five of calomel I have added half a grain, continuing to give them together in the same proportions, night after night, as long as they are needed. Then, on each succeeding day, when a large purgation of the bowels has been duly obtained, I have still given the opium alone, or with saline draughts, in doses of half or one-third of a grain, every five or six hours. And thus, with the larger quantity at night, and the smaller quantities during the day, about two grains of opium have been commonly taken in the course of twenty-four hours.

Here, then, the vascular system, and the nervous system, and the abdominal viscera are all at the same time made to feel sensibly the impression of the remedy, but none of them is subdued by it. And while blood-letting, and opium, and calomel



with purgatives are all made confederate for the cure of the disease, none of them is given in excess.

Now, I do not pretend to say, that this is just the measure, and just the relative proportion in which these several remedies need always to be employed for the cure of acute rheumatism. There are circumstances which would require them to be varied. But, apart from the patient, they cannot be represented intelligibly. As of venæsection, so of these other remedies, after the propriety of their use is already understood, the skill of using them remains to be learnt; the skill which sees when to give a little more, and when a little less, often or seldomer—when to bear heavily or lightly on the bloodvessels, when heavily or lightly on the nerves—and when to obtain larger or smaller purgation of the abdominal viscera. This is the skill which cures diseases and saves lives. And no man ever had it, who did not obtain it from his own self-teaching amid the emergencies of actual practice.

There is a remedy much used, and of unquestionable benefit in the treatment of acute rheumatism, which I must not omit to mention. I have not mentioned it sooner, having not been able to find a place for it among the remedies hitherto spoken of; for its curative properties are not, like theirs, constantly annexed to any known operation upon particular organs. But a

remedy may have as just a claim to our confidence from our bare experience of its doing good (that experience being sufficiently large), as from our perfect insight into its mode of operation.

Let it be observed, however, that any remedy which, working in the dark, is nevertheless trusted for its ultimate effects, requires to be administered with the greatest care. Even *because* it works in the dark, therefore, whenever we venture to give it, the conditions of the malady should always be clearly those in which it is known to do good; and further, *because* it works in the dark, therefore all the possible ways in which it may do harm should be foreseen and guarded against.

With these remarks, I will proceed to tell you what I know of the use of colchicum in acute rheumatism, for it is the remedy that I allude to.

Colchicum, single-handed, cannot (I think) be safely trusted for the cure of acute rheumatism in the severer cases, but it can in the milder ones; and I have so trusted it: yet I do not recommend the practice. Colchicum given alone has been slow, even in these milder cases, of making its curative impression. Many days have generally elapsed before it has produced any abatement of swelling and of pain, of vascular action and of fever; and then, not until it has *begun to purge smartly and even painfully*. Now these are hardly satisfactory conditions upon

which to obtain the remedial effects of colchicum. For to purge by colchicum is to make it act as it does in its first degree of poisoning.

Finding, then, that in the milder cases I had no fair chance of obtaining from it the virtue of a remedy without running some hazard of it as a poison, it was much too dangerous an experiment in my eyes to commit the treatment of acute rheumatism to it mainly or entirely in the severer cases. For now it must be pressed nearer and nearer to the verge of poisoning in order to bring it at all within the capacity of curing.

But in all cases of acute rheumatism, both mild and severe, the practice prevails of giving colchicum, not alone, but as an auxiliary to other remedies. To bleeding and opium and calomel and purgatives, given in the manner specified, many would add colchicum. They would prescribe a grain of the acetous extract, or fifteen or twenty minims of the wine twice or thrice a day, some considering it to act sedatively and as a special auxiliary to opium, and some specifically and with the force of an antidote, as it does in gout.

I, too, use colchicum in acute rheumatism, but not after this manner. I reserve it for special emergencies; and then I employ it with a trust and confidence which I have in no other remedy.

When by venæsection and by opium and

calomel with purgatives, excess of vascular action, and fever and pain and swelling are abated, yet none of them are entirely abolished, but all still linger; or when pain and swelling do not subside at all in proportion to the abatement of vascular action and fever, which are considerably reduced, then I invoke the aid of colchicum, and give twenty or five-and-twenty minims of the wine of the seeds or the root, twice or thrice a day, and I often find the disease proceed uninterruptedly to its cure.

Again, when by the same ordinary course of treatment fever, pain, and swelling have been made to cease entirely, and have suddenly and unexpectedly returned, then I invoke the aid of colchicum, and give it in the same way; and a few doses are commonly enough to dissipate the returning disease, and restore the conditions of health. This is a pure case of relapse. The relapse, however, very seldom reaches the severity of the original attack.

Now on all such emergencies I have been accustomed to administer the remedy quite alone, uncombined with either alkali or opiate, so that the benefit which has resulted has been without question exclusively due to it; and not only exclusively due to it, but due to it purely as colchicum in virtue of that mode of action (whatever it be) which specifically belongs to it. For the cure has followed suddenly, and not waited for



any intermediate operation of the remedy upon the stomach and bowels.

Thus I have gone as far as it is safe or profitable for you that I should attempt to go into the treatment of acute rheumatism. The rest you must learn by watching the actual management of cases in the hospital. I can tell you how to manage *a disease*, but not how to manage *a case*. One man may learn the principles of an art from another's discourse; but he must learn its practice, not from hearing him talk about it, but from seeing him in the act of exercising the art itself.

I must not dismiss, however, the treatment of acute rheumatism altogether without making one or two additional remarks, for fear that in time to come you may fancy that I have not represented the matter quite fairly and honestly. When some years hence you have gained abundant experience of this disease for yourselves, I can well imagine you commenting upon me and my practice thus:—"It is all very well to talk of bleeding, and opium, and calomel, and purgatives, curing acute rheumatism, and setting patients upon their legs in a week or two; so they will, in many cases. But cases are frequently occurring, in which neither singly nor conjointly will they do any thing of the kind."

Now this I believe to be the common fault of writers and lecturers, and of all who in any manner, or any where, undertake to teach practical



medicine, except at the bedside of the patient, that they give much too favourable a representation of their subject. And it arises after this manner. In whatever they say respecting methods of treatment they proceed in the meanwhile upon the assumption, that they have always *a good case* to deal with, and are always called in at the right time. Not only do they proceed upon this assumption, but they do it without saying so.

I can easily understand how, in order to give an intelligible account of the powers of medicine, it may be necessary to display it in operation upon states of disease which are most susceptible of its good impressions. But the teaching which proceeds upon these terms, whatever other merit it may have, is not the type of actual practice. For in actual practice there is no such thing as choosing your own cases—you must take the good and the bad as they come.

Thus there are cases which present their indications of treatment so clearly and prominently, that medicine is sure of its aim, and in which all the conditions of time and opportunity are so favourable that, rightly directed, it cannot fail of success. These we call, in our peculiar language, *good cases*; meaning that they are such as we like to see and like to treat, and have an interest and hope in, and expect our reward in their speedy cure.

But there are also cases in which there are no

indications prominent or clear enough to become the special scope of practice, and in which time and opportunity have been postponed or lost, so that medicine is never sure of its aim; or, if it should happen to take the right one, it could hardly expect to reach it. These we call *bad cases*. We dislike them, and flinch from them, and can only bring ourselves to treat them as a matter of duty.

Acute rheumatism has its good and bad cases; its cases, in which the right treatment is seen clearly and instituted confidently, and pursued in full expectation of success, and success follows; and its cases, in which the right treatment is dimly discerned from the first, and the treatment which is adopted doubtfully, is pursued distrustfully, and ends in failure, or in a distant, tardy, and precarious restoration. Of these last I can scarcely do more than tell you that there are such cases—I cannot describe them.

Where the constitution of the patient is habitually cachectic, and he has no natural health to oppose to the casual incursions of disease, if that disease be acute rheumatism your treatment is apt to fail. Again, where the patient is seen late, and treated late, as in other diseases, so in acute rheumatism the best medical management is often unable to compensate for loss of time, and thus it often fails.

In turning over my records of this disease, I

find that very few cases came under observation and treatment during the first week; that the great majority were admitted into the hospital during the second, and as much towards the end of it as the beginning; that many were admitted in the course of the third, and some not until the fourth week, and even later.

Again, independent of any fault in the natural constitution of the patient, or any mischance of time and opportunity in the application of the remedy, there are certain forms of the disease in which it bespeaks itself less amenable to medicine. There is a form of acute rheumatism in which pain, from its extreme severity, is out of all proportion to the accompanying fever and vascular action. And there is a form in which fever and vascular action with most profuse perspiration and miliary vesicles are out of all proportion to the pain.

Not only degrees of pain, but its existence, in any degree, must be taken upon the testimony of the patient. And I have seen a few cases in which the complaint of it has been little or none at all, and the swelling of any joint has been little or none at all; and yet the fever has been characteristic of rheumatism in the highest degree, leaving some colour for speculation whether the disease were not in its essence *a fever*.\* Both these forms,

\* The real distinction between inflammation and fever is found by M. Andral in the constituents of the blood. In

that in which the pain exceeds the fever, and that in which the fever exceeds the pain, are liable to be lingering, and to resist the application of medicine for their relief.

But there is no such thing as calculating the results of medical treatment with certainty. Success and failure run contrary to expectation sometimes in every disease, but most of all in acute rheumatism. Where you would look for failure, you often meet with success, and *vice versâ*. Of rheumatism it may be said generally, that it is less within reach of the remedy in proportion as it is seen and treated at a period more distant from its commencement. Yet loss of time does not augment the difficulties of after-treatment, or diminish the probabilities of its success to the same degree in acute rheumatism as in other diseases of an inflammatory nature. I have often known acute rheumatism of the severest kind have the start of the remedy full ten days or a fortnight, during which nothing whatever has been done for its relief; and, when at length the remedy has been applied, it has been cured as easily and rapidly as I could promise myself that it would have been, had I taken it in hand ten days or a fortnight sooner.

inflammation its fibrin is always in excess; in fever never. Thus acute rheumatism is pre-eminently an inflammation, and is taken altogether out of the category of fevers.—*Hæmatologie Pathologique.*

Surely here is something remarkable enough to make us stop and think of it for a moment. An inflammation of the brain, the liver, or the lungs, would not thus wait our pleasure or our neglect, and be as curable ten days or a fortnight hence as it is to-day. For inflammation in these organs does not stand still. It is progressive from stage to stage, and each succeeding stage carries it further and further away from the remedy. But it is the very peculiarity of acute rheumatism that it *does*, in a certain sense, stand still. All its actions and movements are simply as forcible and rapid as possible, yet does it stand still. All its energy is expended upon one stage, and there is no apparent progression beyond it. A fortnight ago there was great heat, and nervous and vascular excitement, and great pain and swelling of the joints; and to-day the heat, and nervous and vascular excitement, and pain and swelling, are exactly of the same amount as they were at first. There is no more sign of parts disorganised, or parts destroyed, now than then. Verily, it seems as if the disease had waited to be cured all the while.

This peculiar pathological condition may, in fact, be the reason why, after great loss of time, acute rheumatism is still curable, and often easily and rapidly cured.

But further, as in the treatment of acute rheumatism you often meet with success, where from



present circumstances, and the analogy of other diseases, you might reasonably expect failure, so you often meet with failure, where from present circumstances, and the analogy of other diseases, you might reasonably expect success. As there are frequent cases which suffer no detriment from delay, but when the suitable remedy comes, though it come late, are still ready to be cured, so there are cases which reap no advantage from the earliest and best treatment, or from the habitual healthiness of the patient, but are still slow and reluctant to let go the disease. I have seen the joints swelled and severely painful, the whole surface burning hot, and steaming with perspiration, the vascular system acting with prodigious force, and the nervous system wild with suffering for weeks and weeks together in young and robust constitutions, and in spite of remedies seasonably employed to fulfil all rational indications. Here is an assemblage of conditions not to be found (I suspect) except in rheumatism: the conditions of acute and chronic disease are united in the same subject; actions the most rapid and forcible, and sufferings the most severe, yet both enduring for a long and indefinite period. This rheumatism, which is most acute, and at the same time most chronic, has, at length, worn itself out spontaneously, or has ceased under the influence of some remedy, whose mode of operation we know nothing

about — eolehieum, iodide of potassium, conium, sarsaparilla, cinchona.

I wish I had none of these unexpected issues to tell you of. They must disappoint your calculations, and disturb your satisfaction, just when you were, perhaps, beginning to look with complacency on the happy results of certain straightforward methods of practice which dealt in simple and powerful means, and fulfilled plain and intelligible indications, and were said to do their work, upon the whole, quickly and successfully.

But if I undertake to instruct you out of my little book of experience, I hold it but honesty to read it straight through. There is no such thing as turning practical medicine into a well-told tale. Besides, I doubt whether these exceptional cases of acute rheumatism are not those which especially deserve studying, and which promise more than ordinary fruit to the research of the pathologist.

There are in acute rheumatism fever, and inflammation, and pain. And venæsection and opium and calomel with purgatives have abated them, and oftentimes wrought their cure. And it is wise still to direct our remedies rather in the ways that we know, than in the ways that we know not.

But probably there is more in this disease than fever, and inflammation, and pain. And probably

there is more in these remedies than the mere power of softening the pulse, and soothing the nerves, and draining the abdominal viscera of their secretions. As the disease may have an essential element beyond its sensible actions and sufferings, so the remedies may have secret operations beyond those which are seen and palpable. And it may be in virtue of those that they cure, and not of these.

Still we must be content to apportion the remedies according to such of their operations as we understand, and leave them to exercise, as they may, any others which they possess; believing that those, which we do not understand (though more immediately curative), are closely allied with those which we do, and are ruled and regulated by them.

But, in the mean time, we cannot but wonder that, in the same conditions of disease, other remedies have proved unquestionably curative, without any intermediate operations of the same kind. Nevertheless, among several remedies, equally curative in their ultimate effects, but how we know not, it is best always to choose those, any part of whose action we can see and control. In whatever bark we put to sea, let us have a helm to lay hold of, if we can.

Thus much I have thought fit to say of the treatment of acute rheumatism. And all that I have said applies to it as a disease consisting of fever and inflammation of the joints, and of

nothing more. And, indeed, acute rheumatism, as far as the knowledge of former times went, was a fever with inflammation of the joints, and nothing more. And I heartily wish that the observation of our own times had added nothing to it. But I am not lamenting the fruits of clinical and pathological research, or desiring to recede within the limits of a narrower knowledge. I am only regretting that the things themselves, which modern physicians have discovered, should be discoverable, or in other words, should have a real existence. For they add so much to the perils of the disease, and the perplexities of its treatment, that for my own part I would rather suffer a typhus fever than an acute rheumatism.

Now all these perils and perplexities arise from endocarditis and pericarditis being oftentimes found to form a part of it. But how to form a part of it? Why, I believe that the popular notion regards the disease as it was formerly regarded, as a fever with inflammation of the joints, and that it views the endocarditis and pericarditis, whenever they occur, as mere accidents. But I cannot think so. On the contrary, I must believe that the rheumatism, of its own nature as a disease, engenders the endocarditis and pericarditis, otherwise I could not have deemed it necessary to dwell so long upon its treatment before I came to the treatment of them.

Let me here, in as few words as possible, give

you and myself a little caution against being un-awares led to take various opinions and beliefs in medicine for settled truths, because the terms in current use among medical men would imply that they are so. Endocarditis and pericarditis are commonly spoken of as *incidental* to acute rheumatism. But beware of language, for it is often a great cheat. An incident is something that possibly may happen—a casualty. But whether endocarditis and pericarditis arise in two-thirds, or in one-half, or in not more than one-third, of all the cases of acute rheumatism which occur, still to say of them that they are possible casualties, would lead people greatly to underrate their frequency, and greatly to underrate, too, the amount of peril which belongs to the disease, entirely from their being a part of it. Besides, to speak of them as incidental, seems to settle at once that their relation to the rheumatism is of a certain kind. But who shall say that endocarditis and pericarditis are not equally *essential* to it with inflammation of the joints, and that both are not equally derived from the attendant fever? Or who shall say that the arthritic inflammation and the cardiac inflammation, and the fever itself, with its profuse and sour-smelling perspirations, and the urine, loaded with lithates and red colouring matter, do not all spring from some noxious principle formed in, or finding its way into, the blood, this last containing, in truth, the *essence* of the disease?



Organic chemistry alone can settle the point, and probably will settle it some time.

In the mean while we are called to treat acute rheumatism, and all that belongs to it, upon such knowledge as we possess. But in so doing, let us not deceive ourselves into believing that we know more than we do, merely because we choose to adopt terms which seem to imply great knowledge. "Incidental" and "essential" are words which have a show of wisdom. But we are not sure that the phenomena of the disease to which they are applied can, with propriety, bear to be so designated. The words vouch for more than at present is actually known.

## LECTURE XII.

PREVENTIVE TREATMENT OF RHEUMATIC ENDOCARDITIS AND PERICARDITIS CONSIDERED.—IN THE MANAGEMENT OF ACUTE RHEUMATISM CAN ANY REMEDY BE USED AS SPECIALLY PREVENTIVE OF, OR ANY REMEDY BE AVOIDED AS SPECIALLY CONDUCTIVE TO, ENDOCARDITIS AND PERICARDITIS? — IS OPIUM PREVENTIVE? — IS VENÆSECTION CONDUCTIVE? — IN WHAT THEIR PREVENTIVE TREATMENT REALLY CONSISTS.— THEIR ACTUAL TREATMENT.— IT SHOULD BEGIN WITH THE EARLIEST NOTICES OF THEIR EXISTENCE.— WHAT THESE ARE IN THEIR SEVERAL VARIETIES.— WHY, IN ACUTE RHEUMATISM, THE HEART, BEING INFLAMED, NEEDS A SPECIAL TREATMENT, WHILE THE JOINTS BEING INFLAMED NEED IT NOT. THE TREATMENT OF THE HEART, HOWEVER, BY THE SAME REMEDIES AS THE GENERAL DISEASE.— BUT THIS TREATMENT MADE SPECIAL BY THOSE REMEDIES BEING USED WITH A DIFFERENT FORCE, AND IN NEW DIRECTIONS.— BLEEDING AND OPIUM USED THUS.— THE CONSIDERATION OF THE USES OF MERCURY POSTPONED.

WHENEVER endocarditis or pericarditis has made a part of acute rheumatism, it has rarely been present from its commencement, but has in almost every case been superadded to it during its progress. There have been fever and inflammation of the joints for an uncertain period, before the affection of the heart has arisen.

I say, in *almost every case*. For I have known

exceptions: I have seen cases in which the inflammation of the heart has been declared as soon as the inflammation of the joints: even as soon as the disease could be called rheumatism, endocarditis or pericarditis has been already a part of it. And I have seen a few cases (but very few) in which the inflammation of the heart has seemed to precede the inflammation of the joints. There has been fever, and with it palpitation and præcordial pain. Thus far the disease has been a puzzle. In a day or two the joints have become inflamed, and shown the disease to be rheumatism; and the endocardial murmur has been added to the palpitation and to the præcordial pain, and shown the sure existence of endocarditis from the beginning.

But beyond all question in the vast majority of cases, where endocarditis or pericarditis has eventually appeared, the rheumatism has begun, and proceeded for a time without any symptoms immediately referable to the heart.

Now, if experience has found any treatment which is preventive of endocarditis or pericarditis, this is the time for carrying it into effect—the time of the heart's immunity, which would thus become the most eventful and most important period of the disease.

Whether there be any special safeguards or modes of rescue, that can then be put in force, I will consider presently. But, alas! I must first

tell you that *the time* is too often past before the disease is brought under our care and treatment. The poor are the most frequent subjects of acute rheumatism ; and the following record will show in how many the precious opportunity of preventive treatment was already lost before they reached the hospital.

Out of the 63 cases of rheumatic endocarditis the endocardial murmur was found to exist upon the patient's admission in 27 ; that is, in 3 cases out of 7. And out of the 18 cases of rheumatic pericarditis the exocardial murmur was found to exist upon the patient's admission in 9 ; that is, in one half. Further, out of the remaining 45 cases of one or other of these diseases, although upon admission the sure auscultatory signs were present in none, yet in many there were præcordial pains, or palpitation, or irregular pulse, or catching of the breath ; and it was often part of their history, that they had existed some time previously. But where these were, there the endocardial or the exocardial murmur was sure to follow, thus denoting infallibly the nature of the disease, and at the same time plainly interpreting the meaning of the symptoms which preceded them, and showing that they were derived from the same endocarditis or pericarditis with themselves.

I have already shown that, from the condition in life of those who are most apt to suffer it, a considerable period is wont to elapse after the

acute rhenmatism has begun, before it is subjected to medical care ; a loss of time and opportunity often unpropitious to the treatment of the entire disease, and (it now appears) still more disastrous in prohibiting the use of all possible means to prevent those affections of the heart which impart to it its most formidable character.

But still acute rheumatism is presented often enough to our observation unaccompanied by these affections of the heart to allow the fair trial of means for preventing them, if any such there be. Now, I believe there *are* means which answer this, among other purposes ; but *no special* means to answer *this special* purpose.

I am aware that the treatment of acute rhenmatism by large and often-repeated doses of opium (much larger doses and oftener repeated than I have been accustomed to employ) has obtained the high recommendation of being a safeguard to the heart. Nothing, however, is more difficult than to reach satisfactory conclusions upon points of this kind. Only consider what a vast number of comparative trials is needed to prove that one remedy has greater power than another to mitigate or arrest even *actually existing* symptoms ; and then think of the infinitely greater number of such trials required to show one remedy more efficacious than another in preventing certain symptoms, which possibly might have been, but which never actually appeared.



On the other hand, there is a remedy which I have heard emphatically denounced as inadmissible in the treatment of acute rheumatism, because it favours the metastasis, or the extension of the disease from the joints to the heart : this remedy is venæsection. And if, in my belief, the imputation were true, I ought to warn you seriously against its use. For, practically, preventive caution and preventive treatment are the same thing.

But I believe that the omission of venæsection might be denounced with as much justice as the practice of it for increasing the liability of the heart to suffer in the progress of acute rheumatism. The truth is, venæsection can neither be commended nor discommended *absolutely* on any such grounds. It guards the heart, or it exposes the heart, according as it is or is not employed in proper season and in proper measure.

All that has been said of the right and wrong use of venæsection in the management of the general disease might now be repeated, with the particular aim of preserving the heart untouched. Omit venæsection altogether, where the patient is young and vigorous, and where pains, and swelling, and fever, and vascular action, and all that characterises inflammation, are in excess, and you fling away the remedy of greatest power both for curing what is, and for *preventing* what may be. All other remedies put together cannot compensate the loss of this, whether for cure or for pre-

vention. Nay, all other remedies, for want of the preliminary impression which this alone can exercise upon the blood-vessels, will, each of them, fail of their proper efficacy, and be almost as if they had not been employed at all. Thus, the inflammation is both unmitigated in the parts where it is, and unrestrained from going wherever else it will; and so it goes, whither it naturally tends, to the heart. All this I have seen, and know.

Or employ venæsection recklessly and beyond the necessity of the case, and in great excess, and you may cure the rheumatism at once, or you may so abate the force of all its symptoms as in a manner to suspend it. But, whether you cure it or suspend it, it must be by a *shock*. And, if you do but suspend it, as the constitution recovers from its shock, and as the disease returns afresh, there is a perilous chance that it may not be exactly what it was before, but something worse. Before the unsparing venæsection it was a fever with inflammation of the joints only; in lighting up anew after the venæsection, it may be a fever with inflammation of the heart as well as of the joints. This also I have seen.

There is a lesson which we are apt to learn slowly, but which all of us come to learn at last. It is this—that while present pain and present peril call loudest for relief and rescue, still in

relieving and rescuing, the ultimate well-being of the patient must not be disregarded altogether.

To compare great things with small, it is not only in the art of war that an imprudent victory has been the beginning of many disasters.

Whatever be upon the whole the best treatment of acute rheumatism, the same may be considered the best safeguard against extraordinary perils belonging to conditions which may arise in the course of its progress. While it promotes the cure, it tends to restrict the disease to the parts which it at present occupies until the cure is complete. And whatever be the worst treatment of acute rheumatism, the same is the surest exposure to those perils against which the best treatment is a safeguard. While it delays the cure, it allows the disease time to travel beyond the parts which it at present occupies to others, which in its own nature it is liable to seize upon.

But suppose the heart to be *actually* attacked with inflammation, what is to be done? I am sure I cannot tell you summarily, and in the gross, what is to be done. I must first bring together some circumstances and conditions of actual practice, and place you (as it were) in the midst of them, and then teach you how to act when I have taught you how to estimate the emergency.

Take a case which you have watched and treated day after day; a case in which day after day you have examined and listened to the chest,

fearing and guarding against surprise, and have felt all possible confidence that hitherto the heart is free. In such a case pain arises, distinct and sudden pain in or near the præcordial region; or the heart suddenly begins to beat with excessive impulse; or the heart, as if struck with weakness, suddenly begins to flutter and act irregularly. But withal your ear detects neither bellows-murmur, nor sound of attrition. What then are you to think of the disease, and what to do? You are to conclude that inflammation is begun in the heart, and you are to take measures to subdue it without a moment's delay.

Pain in the heart; excessive impulse of the heart; irregular action of the heart; any one of these, or any two, or all of them together, coming on in the course of acute rheumatism, make inflammation of the heart so nearly certain, that it would be folly to suspend the remedy by waiting for more certainty, and so running the hazard of having a more advanced and less tractable disease to deal with.

But what is it you would wait for to make the fact of inflammation certain? The auscultatory signs, perhaps. And I may be told that I cannot be absolutely sure without them. But there are cases in which it is your duty to be *practically* sure, before you are sure by *the card*; and this is one of them. You would not sacrifice men's lives to the vanity of diagnosis. The auscultatory

signs, when they come, will only serve to localise the disease. Its nature is plain enough already ; and we treat *its nature*, and not *its seat*.

The truth is, if the heart be to suffer inflammation, it is a most happy circumstance, that the first notice of it should be thus declared. Happy, however, only, if such notice be understood and acted upon.

But do not imagine that I wish to disparage auscultatory signs, because I here so strongly insist, that you should trust to other signs, and proceed without them. Presently you will see how entirely I confide in them.

Take another case of rheumatism, in which you have been daily upon the watch for the least sign of evil befalling the heart, and hitherto have found none : no pain, no excess of impulse, no irregular action. In such a case, when you least expect it, you may hear something unusual in the heart. It may be its systolic sound prolonged in an undue degree, or a little harsh, and nothing more. Yet this, if it be really so, even this alone is enough to call for instant measures to subdue inflammation of the heart. For it is almost certain that by to-morrow, this sound, a little more prolonged, and a little harsher than natural, will become a genuine bellows-murmur, and declare the sure existence of endocarditis.

Indeed I am not over-refining in this matter, and those who are in the habit of attending my



visits to the wards know that I am not. They know, too, that I have taken some pains not to be deceived about it. For it is hard to judge and decide truly upon our smaller perceptions; upon things which reach the senses, indeed, but strike them only feebly and faintly. Yet, when these smaller things have such important meanings as they have here, judge we must, and decide we must, using all caution to avoid error.

When, therefore, in acute rheumatism my ear has told me of a more prolonged and somewhat harsher sound of the heart than natural, I have not been content until I have found another ear (some well-instructed and experienced one) to bear testimony to the same fact, and give me assurance that my own did not err. And then I have ventured to play the prophet, and to foretell that, by the next day, this sound would be a bellows-murmur, and inflammation of the endocardium would be unquestionable. To me it has been unquestionable already, and so, not waiting for to-morrow, I have begun its treatment at once.

Take another case of rheumatism, in which neither præcordial pain nor excess of impulse nor irregular action have hitherto drawn attention to the heart, nor do they now. But now your ear may begin to admonish you of something belonging to the heart, which was not there the last time you listened: a sound which is no mo-

dification of what is found in health, but is entirely new, and unnatural. This sound may be as yet audible in so small a space of the præordial region, that, of several who have been carefully listening to the patient's chest during the last half hour, one only has happened to light upon it. Yet is it so sure and certain a reality, that no sooner is attention pointed to the spot, than all can hear it, and all have no doubt of it. The sound is an indefinite sound still; each person who hears it likens it to something different. It is not such as can fix disease either upon the endocardium or the pericardium. But coming on in the course of acute rheumatism, it must be taken for an announcement that all is not right in the heart, that it has begun to suffer after the manner to which acute rheumatism renders it obnoxious, and that it is already in a state of inflammation. Therefore the remedy must no longer tarry. Ere a day is gone, this indefinite sound will be changed into an endocardial or exocardial murmur.

Take yet another case in which, from the absence of all symptoms referable to the heart, you believe it to be hitherto unhurt. You listened perhaps a few hours ago, and still found nothing to excite suspicion. And now listening again you hear the endocardial, or you hear the exocardial, murmur completely formed. These sounds, being the concomitants of acute rheumatism, an-

nounce at once inflammation of the heart, and fix its seat either in the membrane which lines it within, or the membrane which invests it without. Here nothing is wanting to the instant and complete disclosure of the disease; and nothing should be wanting to the immediate and efficient treatment of it.

These are the ways in which I have seen endocarditis or pericarditis begin, when I have witnessed their beginning. Things different in kind, a pain, an impulse, or a sound; things great or small, palpable, or hardly discernible, and never discerned but by the practised sense, have given the first authentic notice of these awful diseases. Therefore the eye, the ear, and every sense and faculty, which can convey intelligence of what goes on within a man should be kept upon the watch for these things.

I have learnt the good of knowing, and the evil of not knowing, their import. And truly do I wish that I could live a few past years again, and carry back with me my present experience for the sake of treating again some cases of rheumatic endocarditis and pericarditis, and of treating them better. I fancy I could *now* save some hearts from permanent injury which then I did not save.

If I had to treat certain cases which I have seen, when there was complaint of pain in or about the præcordial region, I should not spe-

culate about its possibly being an intercostal pain, and so delay for a while (as I have) the treatment of the real disease. When there was excessive impulse of the heart I should not wait, moreover, for a certain unnatural sound to tell me what it meant; and when there was an unnatural sound of an indefinite kind, I should not make the treatment tarry until this sound became an unquestionable bellows' murmur, or sound of attrition. Heretofore, I confess, some such cases had not the best treatment at my hands, because they had not the earliest. And they had not the earliest, because I did not clearly discern the earliest notices of the disease; and so it had the start of my remedy.

These confessions I freely make, that you may profit by them. I cannot carry my experience backward, but you may carry it forward. And it is in the hope of some practical good to come from it, that I have thus analysed and exposed it.

But you must not from my own confessions take occasion to blame me. The whole subject was new to clinical research. And, long after much general acquaintance with it had been obtained, those many smaller pieces of knowledge, by which especially practice is perfected, were yet to be picked up and examined, and rated at their just value. Do not blame me, that I did not find them, and take them, and turn them to all their best uses in a moment.



Well! then, now you know the signs, which in acute rheumatism are fit to raise alarm, and to proclaim that all is not well with the heart. And whenever any of these appear, whether auscultatory or non-auscultatory, many or few, great or small, whether enough to indicate the disease, but not to determine its exact seat, or enough both to declare it to be inflammation, and to fix its seat in the endocardium or pericardium, or in both simultaneously, your treatment must at once be directed to the heart. But what is that treatment to be?

It has been already said, that in acute rheumatism the treatment of the joints may be safely merged in the treatment of the general disease. And why so? Because in acute rheumatism inflammation of the joints, be it ever so severe, is not wont to terminate in their disorganisation. The fibrous, cellular, and synovial structures, which compose them, will abide in a state of inflammation for weeks, and suffer the matter which inflammation effuses (fibrin and albumen) to be deposited within them and about them, and yet in the end recover their perfect soundness and integrity.

But in acute rheumatism the treatment of the heart cannot be safely merged in the treatment of the general disease. And why not? Because inflammation of the heart tends to a destructive disorganisation. Every day that it is allowed to



abide and to continue its progress, the heart sustains more and more injury from morbid matter deposited upon it or within it, and its functions are hindered and baffled, and at last abolished; and these functions are vital.

When the question is of the joints, it might be laid down as a maxim of practice, to treat the rheumatism (or the general disease), and let the joints take care of themselves. But when the question is of the heart, the maxim might be stated conversely; to treat the heart, and let the rheumatism take care of itself. For inflammation of the heart, whether endocarditis or pericarditis, being the accompaniment of acute rheumatism, is to be managed by the same methods and remedies as it would be, were it alone and idiopathic. And these methods and remedies are such as might not be employed at all, or certainly not to the same extent, in a rheumatism attended only by inflammation of the joints.

Now I admonish you that I am going to enter into bed-side details. For I can teach you nothing unless I do, and you can learn nothing unless you attend to them. Bloodletting, and mercury, and opium, are your remedies for these diseases of the heart. And so they are for acute rheumatism, irrespective of inflammation of the heart. And so they are for twenty diseases besides. But little practical instruction is conveyed simply by announcing the fact. For in each of

the twenty diseases, nay, in each case of any one of them, they may need to be employed in different modes and measures. Thus they are only *conditionally* curative after all. But is not this almost as much as can be said of the application of any remedy to any disease? Conditions mix themselves with all medical practice. To know the disease and to know the right remedy, are only first steps towards the right treatment. The success, and even the safety, of practice come from knowing things which lie far beyond. Stop here, and you will soon find it much easier to kill a man with the *right* remedy than to cure him.

Bleeding, mercury, opium, the very remedies you used in acute rheumatism, are (I say) still your main reliance, when inflammation attacks the heart; but bleeding in different modes and measures, mercury directed to a totally different purpose, and opium given with more than one single intention.

As soon as inflammation is known or suspected to have reached the heart, mercury must be given without delay. Or should mercury be already in use, as a remedy for acute rheumatism, with the intent of obtaining large evacuations from the bowels, it must at once have a new direction given to it. The irritation it has produced within the abdomen must by all means be pacified, and its constitutional impression must now alone be thought of, — that impression, of which salivation

is the best evidence, of which (as far as we know) it alone, of all remedial substances, is properly and exclusively capable, and which, under favourable circumstances, is largely counteractive of inflammation.

But, inasmuch as that impression of mercury, or salivation, which is the best evidence of it, cannot be commanded within any given time, you must be content to administer it in the manner calculated for this purpose, and wait the event. For no judgment can be formed of its curative effect upon the disease until salivation arrives. And it may arrive in a day or two, or not until after several days, or after a week, or after several weeks, or it may not arrive at all.

You must give mercury, then, and wait until salivation come to tell you what it has done for the disease, or until it is fair to conclude there will be no salivation. And it does, I confess, require strong confidence in this great remedy, thus to give it, and thus to wait for its ultimate effect, when in the mean time it displays no proximate or intermediate effect as an earnest of its curative operation.

But as in actual practice, so in description, we must now leave this remedy for a while, supposing it to be duly given and duly in operation towards its ultimate effect, and return to it again, when we have considered the uses of the other remedies.

Now the other remedies, while they have their ultimate effect, which is to be seen and judged of in the end, have their immediate effects, which are to be seen and judged of from day to day, and from hour to hour. Their ultimate effect is to cure the inflammation. Their immediate effects are to abate pain and anguish, and dyspnœa, and palpitation; to quiet the nervous system, to produce sleep, to moderate fever.

With respect to venæsection, if in acute rheumatism any of those symptoms referable to the heart are present, which have been already mentioned, auscultatory or non-auscultatory, and especially if they have arisen under your own observation, or, though not under your own observation, if they be now present, and you have reason to believe that they have *recently* arisen, then, should the pulse be found to have even a notable degree of that hardness which is deemed inflammatory, blood must be taken from the arm. Should there be any doubt about venæsection, any misgiving whether the inflammatory hardness of the pulse is quite enough to require it, let it be employed nevertheless. There is greater hazard in omitting it wrongfully than in practising it wrongfully.

But with the same amount of vascular action evidenced by the state of the pulse, in a case of mere rheumatism, in which the heart was not



affected, venæsection would be neither necessary nor proper.

Again, if in acute rheumatism, with symptoms referable to the heart and those recently declared, fever and vascular action run very high, and there be extreme fulness and hardness of the pulse, a copious venæsection should be practised. And the same should be repeated, were the hardness and fulness of the pulse found not to yield. And repeated again and again, until the hardness and fulness of the pulse were much abated.

With all this fever and inflammatory action, and all this hardness and fulness of the pulse, had the case been one of mere rheumatism, and the heart unaffected, venæsection would have been properly employed, indeed once or even twice, but it would not have been carried further for reasons formerly assigned. Now, however, more danger is to be feared from the progress of inflammation in the heart, than from any shock imparted to the nervous system by loss of blood.

But though the disease be inflammation, and the organ inflamed be a vital organ, even the heart, bleeding must have a limit; for bleeding cannot alone be trusted to cure the disease. Mercury must become its auxiliary. And if for no other reason than to obtain from mercury a fuller curative operation, bleeding must have a limit.

Looking, as I do, mainly to mercury to save



life, or to save the organ, I am constantly careful in the management of every case to do every thing to aid, and nothing to hinder, its curative operation. Especially in the use of venæsection, I bear this in mind; for sure I am that loss of blood can both aid and hinder it according to its amount.

In men of florid aspect and full bloodvessels, though *bleeding* has not been needed for its own sake, yet has it oftentimes been *moderately* used for expediting the sensible effects of mercury. And the sensible effects thus induced have been at the same time curative. But, if the body be first made exsanguine by the lancet, you may gain the *sensible* effects of mercury, and lose the curative; for the two do not of necessity go together.

As mercury can be less trusted for its antiphlogistic power in those who either naturally or from previous disorder are pale and anæmic, so in the robust and sanguineous if you would have mercury exercise what power it has for the control of inflammation, you must beware of *making* these what the others *are*. You must hold your hand from excessive venæsection. In inflammation of the heart you must not first change a fine ruddy countenance into the aspect of a chlorotic girl, and then leave mercury to complete the cure without anxiety or distrust of the event.

Recollect, then, that in rheumatic inflammation

of the heart, whether it be endocarditis or pericarditis, bloodletting by venæsection is to be little in one case, and much in another, according to the present force of vascular action throughout the body; little or much respective to its own proper benefit as a remedy; and little or much respective to its secondary uses in procuring the more effective operation of mercury. And recollect, moreover, the cautionary admonition applicable alike to the management of this and of all diseases in which a large share of the cure is confided to mercury; recollect, not to lessen or to lose its curative operation, either by bleeding too little or bleeding too much; for plethora and anæmia are alike obstructive of it.

But there are other modes and means of bloodletting besides venæsection. There is cupping, and there are leeches.

Now in rheumatic inflammation of the heart, cupping or leeches, or both, may be needed as auxiliary to venæsection; or, exclusive of venæsection, they alone may be needed and trusted for taking as much blood as the nature of the case requires.

As it often happens to other organs when they are inflamed, so also to the heart, that, when general vascular action has run high and venæsection has been employed to reduce it, and *has* reduced it effectually, the local symptoms will remain altogether, or almost unmitigated. Thus

pain and palpitation of the heart, and unnatural sounds, often abide in the same degree, or nearly so, after venæsection as before it; and then cupping or leeches come in as effective auxiliaries, and the local bleeding often makes at once an appreciable impression upon what the general bleeding has not touched.

Again, as in other parts of the body, so in the heart, it is no uncommon thing for inflammation to begin and proceed onwards to destructive disorganisation *without being felt as an inflammation* by the constitution at large. It imparts no special hardness to the pulse, and no extraordinary force to the circulation, yet is it working its own injurious changes locally; palpitation, or pain, or irregular action, or unnatural sounds, declare as much. Here all that bloodletting can compass for the relief of the disease and its symptoms will be attained by cupping, or by leeches alone.

Now there is a choice between cupping and leeches. One may be a more appropriate remedy than the other in a particular case; and yet I dare hardly trust myself, apart from the patient, to point out what it is that should determine you to prefer one to the other.

So dexterous are those who are well practised in the art of cupping that they can make their glasses draw a given quantity of blood in almost as short a time as the lancet; whereas leeches are long at work in taking away the quantity you

desire. Therefore, upon the principle of requiring in the remedy a force and rate of operation proportionate to the force and rate of the disease, I should say that when the pain or anguish, or by whatever name you call the distress immediately referable to the heart, begins suddenly, is at once felt severely, and augments rapidly, then cupping is the remedy; but that, when it comes on by little and little, and increases slowly, and has not yet reached a great amount, then leeches are the remedy. But leeches are often needed as auxiliary to cupping, just as cupping is to venæsection.

There is another thing worth mentioning as to the use of these remedies. When I have employed cupping in inflammation of the heart, I have been accustomed to have the glasses applied between the left scapula and the vertebral column. Applied upon the præcordial region, they have seemed to me to cause peculiar distress, and to be not without the hazard of doing mischief and violence to the heart in its present condition: for recollect that *now* a slight percussive upon the cartilages of the ribs, or any degree of pressure which shall carry them ever so little downwards towards the heart, will often occasion pain. Besides, blood taken from between the scapula and the spine produces all the relief to the heart which could be expected.

But we must come nearer yet to the bedside of the patient, and look still more closely into the



immediate effect of bloodletting in inflammation of the heart. Its effect, day by day, is the earnest of its ultimate effect. The sensible impression of each venæsection, or of each cupping, or of each application of leeches, is a measure of the good they are doing, and of the trust to be reposed in them as remedies. But what is this effect which is to be looked for day by day?

In inflammation of the lungs or pleura, bloodletting shows its favourable impression by setting free the respiration and diminishing pain. In inflammation of the brain, by abating delirium, or coma or spasm, by restoring clearness to the senses, and intelligence to the mind; in inflammation of the peritoneum, by softening the tense abdomen, and making it more patient of pressure. And if the heart be inflamed either within or without, any of the several modes of bloodletting will denote itself to be exercising a curative power upon the disease, when it moderates pain, or when it moderates excessive impulse, or when it renders its beats regular again, which were irregular before. Thus it is in the sensations and functions of the heart as of other organs, that we see the benefit which bloodletting is doing, or is ultimately to do, towards the cure of its inflammation.

Next as to the use of opium. It indeed is needed now that the heart is inflamed, to calm the nervous system, and to abate pain, as it was needed before; and, moreover, it is needed for another



purpose, viz. to give effect to the operation of mercury in one mode at least of administering it.

If in aiming to produce salivation you give calomel internally, you must restrain it from running off by the bowels: and this can only be done by opium. Opium must be combined with every dose of calomel, and the quantity of each must be proportioned to the other. Thus, perhaps, we shall generally find ourselves compelled to give more opium for the purpose of keeping the bowels patient under the stimulus of calomel, than we should give merely to soothe the nervous system under the general irritation of the disease. But, as the remedial operation of mercury is not now under consideration, it is unnecessary to say more at present of opium as its adjunct and auxiliary.

Beside, however, these its common uses of calming the nervous system and abating pain and aiding the effect of mercury, opium has its extraordinary uses for extraordinary emergencies. In endocarditis and pericarditis, not always, but most commonly, pain is present; pain of the heart varying in amount, and upon the whole abiding, yet allowing remissions and abatements. But whether this customary pain be present or no, a sudden agony will sometimes seize the heart, and at once the patient will feel and look as if he had received his death-blow. It is like a spasm of the heart. It is no other than a paroxysm of angina

pectoris. This terrible seizure attends pericarditis more frequently than endocarditis, and where it has once occurred, it is apt to occur again. Its only remedy, as far as I know, is a large dose of opium. Opium is the only means of rescue during the agony, and the only safeguard against its return.

Thus it is with opium as it is with bleeding. Day by day it makes its sensible impression both upon the constitution at large, and upon the diseased organ. It is pre-eminently sedative and anodyne and antispasmodic. And so day by day it is curative in a certain measure, and gives assurance that it is contributing in the mean time to the ultimate cure.

Well, then, while we look to *feelings and functions*, both constitutional and local, and gather notices from them that our remedies are working beneficially towards their end, have not the auscultatory signs some intimations of the like import to give us? Having gained great information from them respecting the nature and seat of the disease within the heart, and the period of its commencement, we are disposed to look to them to tell us of its decline and reparation. And no doubt they do tell us a great deal ultimately. They give us all the knowledge that can be gained of the condition to which the heart reverts at last. But in the mean time, are not changes to be sought for in the auscultatory signs day by day as

bleeding after bleeding, or remedy after remedy (be it what it may) is employed, changes which shall show the curative effect of the treatment now in progress?

Experience says that changes in the auscultatory signs do not occur after this manner; and the nature of the thing shows that they cannot.

By venæsection or other modes of bleeding, by a blister or by a dose of opium, pain, anguish, or palpitation or irregular action of the heart, are subdued or pacified, or made more tolerable; and the same symptoms returning, are again brought under by the same remedies. But in the mean time, the murmurs, endocardial or exocardial, which have been present from the beginning, do not come and go, and rise and fall. When pain or anguish, or palpitation, or irregular action of the heart, is the greatest, the unnatural sounds are not the loudest. When those are the least, these do not become faint or inaudible. So says experience.

Now only consider how different in their very nature are *those* symptoms and *these* auscultatory signs, and wherein that difference consists, and you will presently see how it comes to pass that they cannot both be brought under equal degrees of abatement and control by the same remedies.

Pain, anguish, palpitation and irregular action, are things purely vital. They are the sensibilities and functions of the organ exalted and hurried,

and baffled by the present stimulus of inflammation. And so far as the remedy can subdue it partially or entirely, temporarily or permanently, they are made to subside or to cease for a while, or altogether. But the endocardial and the exocardial murmurs are annexed to purely mechanical conditions, and proceed from new substances formed within and without the heart. And as long as these substances remain, whether the heart move forcibly or feebly, rhythmically or irregularly, with pain or without pain, it cannot move without the accompaniment of these sounds.

It is true that these substances are formed by the bloodvessels engaged in the inflammation. But the products of the inflammation remain for a time after the actuating principle and vital movements of the disease have ceased. And as long as they remain they have *mechanical* effects according to their kind, their seat, and their degree. As long as lymph abides upon the internal lining or external covering of the heart, though inflammation have entirely ceased, unnatural sounds must result. As long as the blood has a thickened valve, or an unequal surface of the endocardium to pass over, it must pass with a whiz. As long as the folds of the pericardium, being still free to move, encounter ruggedness and inequality, they must move with a grating noise.

Learn, then, to read aright the meaning of these two orders of symptoms referable to the heart,—the

vital and the mechanical. Beware, especially, when the question is concerning the effect of remedies upon the disease, of mixing or confusing the intelligence which each has now to give. The vital symptoms, when they cease, denote that the inflammation has come to an end. The mechanical symptoms, when they yet continue, denote that the effects of the inflammation remain. The two do not speak a contradictory language, but they speak of different things.



## LECTURE XIII.

THE GENERAL QUESTION CONSIDERED OF MERCURY BEING A REMEDY FOR INFLAMMATION.—CONDITIONS FAVOURING ITS REMEDIAL OPERATION. — THESE FOUND IN THE NATURE OF THE INFLAMMATION, IN THE PART IT OCCUPIES, AND IN THE CONSTITUTION OF THE PATIENT. — ITS REMEDIAL OPERATION EITHER ANTIPHLOGISTIC OR REPARATORY. — THE FIRST CHIEFLY DISPLAYED IN THE INFLAMMATIONS OF TROPICAL CLIMATES.—PARALLEL FORMS OF INFLAMMATION HARDLY KNOWN IN THIS COUNTRY.—WHAT COME NEAREST TO THEM.—THE EFFECT OF MERCURY IN THESE NOT ALONE, BUT CONJOINTLY WITH BLEEDING.—THE PROBABLE NATURE OF ITS ANTIPHLOGISTIC OPERATIONS INFERRED FROM ITS EFFECTS, ESPECIALLY IN TWO KINDS OF INFLAMMATION. —ITS REPARATORY OPERATION SHOWN BY INSTANCES.

As a further remedy in the treatment of endocarditis and pericarditis, I would proceed next to speak of mereury. But I find myself unable to manage this part of the subject intelligibly, without first making you acquainted with my notions of the use of mercury in inflammation generally.

Upon a matter like this more uniform and settled opinions than we find might have been expected among medical men. Some appear to regard mercury as absolutely remedial of inflammation, and apply it somewhat indiscriminately

and extravagantly, from a too credulous belief of all that has been reported in its praise. And some see nothing remedial in it, and forego its use altogether, because they look for a more exact measure of its benefit in each particular case than it is possible to obtain.

But men of experience surely cannot differ thus widely: I mean that sort of experience which belongs to the subject.

Those who are largely conversant with inflammation in its graver forms,—such as it often appears in hospitals,—and are much engaged in treating it, must have made or witnessed frequent trials of mercury for its cure. And among such, who possess the needful experience, while there would probably be found different degrees of confidence in it as a remedy, none would deny to it all curative power whatever.

Now I venture to offer the following considerations, hoping that they may serve to reconcile in some degree discordant opinions, and to show *how far* and for *what* mercury may be trusted in the treatment of inflammation.

First, then, it is quite certain that mercury is not applicable to all cases of inflammation alike. In some it is eminently remedial; in some less obviously so; and in some it is not remedial at all. Therefore, besides the fact of the inflammation, there must be conditions annexed to it, which aid or insure its efficacy.

What are these conditions? I do not know that I can clearly point them out. But perhaps I can tell you *where* to look for them.

You are to look for them, then, not only in the inflammation itself, but also in the part on which it falls, and in the constitution of the patient.

The condition which respects the inflammation itself is probably contained in its greater tendency to certain results. Now inflammation tends to the deposition of lymph, and to the effusion of serum and of blood, and to suppuration. And all these results are often found to occur equally and in quick succession, or almost simultaneously; but often one or other is the predominant or almost exclusive result. Accordingly, in different cases, inflammation will bear to be called adhesive, or serous, or hæmorrhagic, or suppurative. And the more it is adhesive, or has its tendency to the deposition of lymph, the more does it admit the curative impression of mercury.

That over and above this predominant tendency of the inflammation itself, there is some condition belonging to the nature of the part, which favours the remedial operation of mercury, would seem highly probable. For all experience bears testimony to its more general utility in inflammation of serous than of mucous membranes. In pleurisy, in peritonitis, we are accustomed to give mercury without much discrimination of the

kind of inflammation we have to deal with, or, whether its predominant tendency be to lymph, or serum, or pus, or blood; and success has attended the practice. But in tracheal and bronchial inflammation we seldom give it, and yet the majority of cases do well without it.

Still there may be a doubt, whether what we ascribe to the part may not be all included in the nature of the inflammation. For in serous structures its tendency is almost always to deposit lymph. In pleurisy and peritonitis, whatever else be found, serum, or pus, or blood, it is all involved in false membranes; whereas in mucous structures its tendency is almost always to mucus or muco-purulent effusions. But occasionally, although very rarely, laryngeal, tracheal, and bronchial inflammation has its sole and entire result in the deposition of lymph; and then mercury becomes the remedy upon which we mainly rely for its effectual cure. The very exception points rather to the nature of the inflammation than to its seat.

But whatever may be thought of one sort of inflammation rather than another, and of one inflamed part rather than another needing mercury and favouring its effect as a remedy, there are states of constitution which aid or hinder its curative operation in a remarkable manner.

Now the constitution which bears mercury the



best, and most readily accepts and appropriates all the good it is capable of doing, is that which is naturally and habitually the most healthy and the most free from all specific taint or weakness, whether hereditary or acquired. When in such a constitution inflammation is met with, it commonly arises from accident; but having arisen, it partakes of the nature of the subject. It is simple and vigorous, and speeds on rapidly to its destined results. Inflammation in such a constitution would never be cured, if the same strength which sustains the force of the disease did not sustain the greater force (for greater it must be) of every counteracting remedy, of bloodletting and of depleting methods in all their kinds, and especially of mercury.

The constitution which bears mercury the worst, and is most apt to convert the good it might do into evil, is that which is habitually unhealthy, and has acquired or inherited some specific taint or weakness, as scrofula. When inflammation is met with in such a constitution, it either proceeds from some slight provocation, or arises spontaneously out of the morbid habit of the subject; and having arisen, it still partakes of the same as it goes along. Its course is unequal and precarious, not steadily tending to any certain result. All remedies applied to it are of doubtful efficacy. The same weakness which is unequal to carry on the disease cannot sustain the force of simple antiphlo-



gistic remedies, and is especially abhorrent of mercury.

But is mercury to be altogether forbidden as a remedy for inflammation in scrofulous constitutions? I am far from saying so. For even in them I have sometimes seen inflamed organs plainly and palpably rescued from destruction by mercury pushed to salivation.

But then the cases were peculiar. The inflammation had been unnaturally vigorous and rapid for the constitution in which it was found, as if it came from some violent irritation forcing for the time a feeble body into actions (as it were) beyond itself, and enabling it to bear for the time extraordinary remedies, and mercury among the rest. Thus phrenitis, peritonitis, or pleurisy suddenly developed and rapidly progressive, in the most scrofulous subjects, should have the chance of benefit from the mercurial treatment.

These, then, are some of the conditions in the disease itself, in the part it occupies, and in the constitution of the patient, which seem to favour the success of mercury as a remedy for inflammation.

Further, some little insight has been gained from experience into the mode of its curative operation.

I believe that mercury has two ways by which it contributes to the cure of inflammation. In the one it constrains the morbid energy of the

Blood-vessels, and counteracts the powers by which the inflammation is carried on. Thus it takes its place in the same rank with blood-letting. In the other, it aids the reparation of parts by promoting the removal of substances foreign to them, whether fluid or solid, which inflammation has produced and left behind. Thus it displays a power different in kind from that of blood-letting, and coming into operation, and having its work to perform, after blood-letting has done all it can. Let us call the first the *antiphlogistic*, and the second the *reparatory*, operation of mercury, and consider each separately.

The purely antiphlogistic power of mercury is displayed most eminently in the inflammatory diseases of intertropical climates. Here the race is fairly run between the actions and movements raised within the body by the disease, and the counter-actions and counter-movements raised by the remedy. The work of destruction by the disease is the work of a day or two. And the work of a day or two must be the work of counter-action by the remedy. Even within this time mercury must be made to salivate, if mercury is made to cure. If the disease outrun the remedy, the patient dies; if the remedy outrun the disease, the patient is saved. And all that respects the disease, and all that respects the remedy, is so marked, so sudden, and so forcible, that physicians neither doubt nor reason about the matter.

They *see* what happens, and, resting upon the evidence of what they *see*, they know that the disease is cured by mercury.

Yet hardly more certain is the fact of the cure by mercury, than the manner in which it exercises its curative operation. For, when mercury arrests and cures these frightful inflammations of the tropics, it is plain that its power is expended upon the *actions* of the disease in controlling them, not upon the effects of the disease in inducing their reparation. The effects, if the disease go on to its effects, are such as admit of no reparation. Organs are found mashed, dissolved, and fallen in pieces, with hardly a trace of their elementary structure left, when the patient dies.

Our knowledge of this purely antiphlogistic power of mercury is new knowledge, almost within our own times. We are indebted for it mainly to our own countrymen practising within the tropics, and a large amount of gratitude is due to them from all mankind.

Now, what mercury could do in India, it was naturally supposed capable of doing in Europe; and experiments were not long wanting to put it to the test.

It was about the time I was a student, that mercury was first greatly talked of and greatly employed as a remedy for acute inflammation in this country. And it has been so talked of and so employed from that time to the present. In

the meanwhile experience has been growing ; and now, perhaps, the use and amount of its antiphlogistic power are settled as far as they ever can or ever will be.

With us inflammation never, or very rarely, begins and runs on and terminates exactly after the pattern of Indian inflammation. It comes nearest to it, when it puts on its acutest form, and attacks vital organs. And in such inflammation mercury has been administered something after the Indian mode and measure, and has obtained results which display satisfactory proof of its antiphlogistic power. So satisfactory, that, for the sake of this power which they believe it to possess, a majority of the most experienced physicians in England at this day would be found employing mercury and pushing it to salivation in the earliest stages of acute phrenitis, pneumonia, pleurisy, and peritonitis.

If I am told that phrenitis, pneumonia, pleurisy, and peritonitis in their acutest forms have been cured by other means, before mercury was yet known to have an antiphlogistic power, or had ever yet been given with an antiphlogistic aim ; that they have been cured by timely and copious blood-letting, and so may be cured again ; I admit the fact. If I am further told, that the stoutest advocates for the use of mercury dare not leave such cases to mercury alone, but employ it concurrently with blood-letting, I admit the fact.

But questions of practical medicine are not to be settled like points of casuistry. The logical inference from the result of certain cases may incline one way, and the general mass of experience may incline another.

In this, that, and the other case of phrenitis or peritonitis, pneumonia or pleurisy, the patient is largely bled and rapidly salivated, and is quickly well; and in all this perhaps you find no sure warrant for believing that mercury contributed anything to the cure; or, perhaps, you infer confidently that the cure was altogether due to the blood-letting; for in other cases bleeding alone has been employed, and recovery has followed.

Well! as far as *the particular cases* go, I cannot venture to say that your inference is wrong. Still my practice must be governed by the sum of my experience. And the sum of my experience is this, that the acutest forms of these inflammations are arrested more surely and more speedily by bleeding and mercury conjointly than by bleeding alone; and not only more surely and speedily, but by a less loss of blood; in short, that mercury does not supersede blood-letting, but that it aids its antiphlogistic power, and yet spares its amount.

Now, in the popular notion, the acutest inflammation is that which displays the greatest power and force of vascular action and the greatest febrile heat, and which works its local changes



upon the part affected with the greatest rapidity. Such inflammation we have been contemplating.

But there is inflammation which works its local changes with extreme rapidity, and yet rouses the movements of the general vascular system little, or not at all; nay! even sometimes depresses them, and raises little or no febrile heat. By what name would you characterise this inflammation? As respects its general vascular action and its fever, acute it is not. Yet, as respects its local morbid processes, its effusions of serum, lymph, pus, and its dangerous or destructive disorganisation, acute it is indeed. But, by whatever name you call it, by what remedies would you treat it? If by bleeding at all, it must be by local bleeding, and sparingly and cautiously even by that. There is no great and palpable force of action in the disease, and therefore no great and palpable force of impression needed in the remedies. But there is a destructive disorganising process rapidly at work, which needs to be rapidly overtaken and counteracted. And this (as I well know) is often effected by mercury.

Thus phrenitis and peritonitis, pneumonia and pleurisy are seen to hurry on their destructive processes, making no show of energy and power, and bearing and requiring loss of blood sparingly or not at all; yet curable, and often cured, by mercury.

Now reflect for a while upon what has been said. Here are two forms of acute inflammation, both alike, yet both different, and mercury having a share in the cure of both, but not an equal share. Think what these inflammations are, and *how* mercury is brought to bear upon them, and perhaps you will gain some little insight into the nature of its remedial operation.

In both, the essence of the disease within the part is the same, equally rapid, equally destructive, and tending to identical results; but the accompanying conditions in the constitution at large are different: in the one great febrile heat and great excess of vascular action; in the other little or no febrile heat, and a defect rather than excess of vascular action. In the first, there is *the* disease, and much beyond it, the result of sympathy reaching to all the nerves and all the blood-vessels throughout the body. In the second, there is *the* disease and *the disease alone*, carrying on its own essential morbid processes within the part. Blood-letting is a remedy for the first, and sometimes the sole and exclusive remedy. It subdues the high vascular action and febrile heat, and, reaching the disease within the part through the constitution, it subdues it also, and so compasses the entire cure. But while blood-letting is a remedy for the first, it may not be the sole and exclusive remedy. It may subdue high vascular action and febrile heat, and not at all arrest the disease

within the part, or it may restrain it a little but not subdue it, and it may need the help of mercury to complete the cure.

Mark then ; it is upon the very essence of the disease, upon the essential morbid process within the part, that the remedial power of mercury is brought to bear.

In the second, blood-letting is a less prominent and a less needful remedy, and mercury has a larger, and a more potential and more intelligible share in the cure. For here, too, and here more unquestionably, its remedial operation is, and must be, counteractive of the disease within the part, and of it alone. For the disease within the part is the whole disease.

Well, then, from the share which mercury seems to have in the cure of these two forms of acute inflammation, what is the little insight (for it becomes us to speak modestly in such a matter) to be gained into the nature of its operation as an antiphlogistic remedy ? It is this ; that, whereas some remedies act upon the heart and pulsating arteries, and so become antiphlogistic by subduing their impulse, or moderating its excess ; others upon the nervous system, and so become antiphlogistic by abating irritation and pain ; and others act upon glands and upon various organs which secrete, and so become antiphlogistic through derivation or sympathy, mercury acts upon the extreme blood-vessels. And

these are the immediate instruments of the disease; and so mercury becomes antiphlogistic by a mode of impression which directly interferes with the inflammatory process itself.

But mercury is reparatory as well as antiphlogistic. When inflammation has not been withheld from reaching some of its results, and these have done injury to the structures it has occupied, injury, however, not altogether irreparable or presently destructive of life, then mercury is among the remedies which have power to call into action those wonderful capacities which nature has provided for her greatest need. It now belongs to mercury among other remedies to aid and hasten and render more effectual those operations of the part and the constitution, by which organs are entirely or partially restored to soundness, and life is saved.

When phrenitis has reached hydrocephalus, or peritonitis abdominal effusion; when pneumonia has reached hepatization, or pleurisy hydrothorax, then mercury is among the remedies which have a power of stimulating the absorbents to remove the matter extraneous to the blood-vessels whether fluid or solid, and so of restoring the diseased structures to the conditions of health. Therefore in this state of things it is high time to give mercury, if it has not been given before; or if mercury has been employed at every previous stage, there is now as much need of it as ever. Whether,



pending the inflammation, the chance of benefit, from the antiphlogistic power of mercury has been thrown away, or whether it has been made the most of, and has not succeeded, there is its reparatory power still to look to, which may make up for the fault of our practice in the one case, and the failure of our practice in the other.

But as, with its antiphlogistic power, mercury has sometimes a smaller and sometimes a larger share in the cure of inflammation; so, with its reparatory power, it has sometimes many auxiliaries, and is sometimes itself almost all in all. There are cases in which, under the use of purgatives and diuretics and counter-irritants, the effused serum or lymph is daily becoming less and less; and when salivation arrives, it is difficult to determine how much mercury contributes to insure or complete the reparation which had already begun, and proceeded to a certain point. And, again, there are cases in which serum and lymph abide undiminished in cavities, on surfaces, and among interstitial textures, in spite of many remedies; yet no sooner does salivation appear than they forthwith begin to be absorbed, and continue to be absorbed rapidly until they are entirely gone.

This distinction between the antiphlogistic and reparatory powers and operations of mercury is simple, and intelligible, and (I believe) true. It has not been plainly stated, but it has been



plainly acknowledged by the common practice of medical men, and confirmed by their experience. I am sure it will be usefully borne in mind. For there are no conditions more essential to the success of our practice than these; namely, first to understand the purposes which a remedy is able to fulfil, be they few or be they many, and then to have clearly in view those for which it is needed in the particular case, and then steadily to point it and press it to its mark.

But, though they may be distinct in kind, it is not always easy to determine where the antiphlogistic operation of mercury ends, and its reparatory operation begins. And truly they are often both required and both displayed in the same case. Where mercury is needed and given to abate or arrest inflammation in its earliest and most active stages, it is commonly still needed and still given in its decline. The best and most efficient treatment of inflammation is seldom so absolutely successful, but that something is left for reparation before the organ can be said to have acquired its perfect soundness.

## LECTURE XIV.

SUBJECT CONTINUED. — ANTIPHLOGISTIC AND REPARATORY POWER OF MERCURY OVER ACUTE INFLAMMATION FURTHER ILLUSTRATED BY ITS EFFECTS IN IRITIS, OVER CHRONIC INFLAMMATION BY ITS EFFECTS IN RHEUMATIC OPHTHALMIA. — ITS EFFECTS UPON INTERNAL CHRONIC INFLAMMATION. — OUR ASSURANCE OF THE SEAT IS GENERALLY GREATER THAN IT IS OF THE ESSENCE OF INTERNAL CHRONIC DISEASES. — OUR CONCLUSIONS, THEREFORE, LESS CONFIDENT RESPECTING THE EFFECTS OF MEDICINE UPON THEM. — EXPERIENCE OF THE CURATIVE EFFECTS OF MERCURY IN MANY HIDDEN DISEASES, WHICH FROM CIRCUMSTANCES ARE DEEMED INFLAMMATORY. — NOTICE OF A PRINCIPLE TO BE REGARDED IN THE MODE OF ADMINISTERING IT.

ALL that has been said of the uses of mereury, both as an antiphlogistic and a reparatory remedy, will be found to have its best illustration in the inflammatory diseases of one particular organ. And that organ is the eye. And if any of you have imbibed an unlueky scepticism respecting the eurative powers which belong to mercury, a month's diligent attendance and observation at the Eye Infirmary will be sure to disabuse you of it. The eye might have been intended to furnish us a little model for studying proeesscs of disease and processes of reparation as they go on in all

parts of the body, so admirably does it answer this purpose.

In the eye we may behold the miniature of all diseases; for here nature has displayed, as in a glass, all the little intimate details of her own wonder-working powers; her modes of disorganising, and her modes of repairing; and the aids which she receives, and the impediments which she sustains, from the right and wrong application of medical agents.

Let us take the iris, its inflammations, and their cure, and dwell upon them for a while. There are cases of iritis in which the entire cure is achieved by blood-letting alone. The fact cannot be denied. But such cases are rare. And there are cases in which the entire cure has been compassed by mercury alone. Neither can this fact be denied. But such cases likewise are rare. The vast majority of cases which have terminated in a restoration of the organ to its perfect structure and functions have been treated by both. Experience is so decisive upon this point, that it would be morally wrong to come to the treatment of any particular case with the purpose of trusting exclusively to either.

Truly there is enough for both to do. And with a little attention you may see clearly what it is that each really does, and may apportion to one and the other the exact share that belongs to it in bringing about the result.

Now blood-letting and mercury have not the same relative share of the cure in all cases. In one, blood-letting has a very large share, and mercury a very small one; and in another, the two remedies have their proportionate shares inverted; while in neither case could either of the remedies be dispensed with.

In very acute iritis where, besides the iris being discoloured and covered or studded with lymph, and besides the irregular pupil and the vascular zone around the cornea, the sclerotic is streaked with blood-vessels and the conjunctiva is as though injected with vermilion, blood-letting is demanded; even venæsection, if the pulse be hard and full, and cupping upon the temples, if it be not.

And how much does the blood-letting in such a form of iritis usually effect? It changes the general aspect of the eye. It empties the blood-vessels of the sclerotic and conjunctiva. All that was vividly red beyond the margin of the cornea becomes paler. But all within this limit remains the same; the zone round the cornea, the lymph upon the iris, and its discoloration, and the irregular pupil all remain the same. Blood-letting has abated or subdued the inflammation so far as it has exceeded the bounds which essentially belong to it as an iritis. But within those bounds it has not reached it remedially in the smallest degree. Within those bounds, in spite of the blood-letting, the inflamma-

tion is still proceeding uninterruptedly to its results. The points of lymph go on enlarging themselves into masses, which more and more fill the anterior chamber, and elose the pupil, and more and more involve iris, and cornea, and crystalline lens in one indiscriminate disorganisation.

But add the use of mercury to blood-letting in the treatment of iritis in this, its acutest form ; and, besides a visible emptying of blood-vessels in the entire organ, there will, as soon as salivation arises, be a visible change in the condition of parts within the proper sphere of the inflammation. Whatever stage the inflammation has reached, there it will pause. Then the cluster of blood-vessels, which tended to the margin of the cornea, will become paler and paler, and the vascular zone will be seen to fade, and the drops of lymph which studded the surface of the iris will cease to increase, and then begin to lessen, and then gradually disappear. In the mean time, the aqueous humour becomes clearer ; the pupil, which was rendered irregular by partial adhesions, recovers its circular form, and vision is perfectly restored.

But in iritis, when the general aspect of the eye is not vividly red, and when the general vascular system is not roused into fever, and there is no hardness of the pulse, and yet, nevertheless, within its own specific limits, the inflam-



mation is carrying on its own essential processes actively, rapidly, and destructively, and the zone encircles the cornea, and the iris is covered or studded with lymph, which is daily becoming more and more, and the pupil is irregular or fixed; then there is small need of blood-letting, and only of local blood-letting at most; but the need of mercury is as great as ever. We cup or we apply leeches; and the good that we thus do is seldom immediately apparent. It is not until the mouth becomes sore, that any visible change begins in the conditions of the disease.

Here, whatever may be due to cupping or leeches, observation cannot be wrong in assigning the larger and more appreciable share of the cure to mercury.

Such is the strong testimony which practical experience can furnish to the antiphlogistic and reparatory powers of mercury, when it is brought to bear upon *acute* inflammation. By *acute* inflammation, I mean that which carries on its disorganising processes *rapidly*, and which works manifest change and detriment within the structure of parts from day to day, whether it be accompanied by much excess of action in the vascular system at large, and much febrile heat, or by little or none at all.

But mercury is capable of an antiphlogistic and reparatory impression upon *chronic* inflammation. By chronic inflammation, I mean that

which carries on its disorganising processes so slowly that the amount of change and injury it does to the structure of parts is not appreciable from day to day. It is also the characteristic of this inflammation to abide long in one stage before it passes on to another.

Now this general tardiness of its progress, and this lingering in its several stages, give leisure and opportunity for learning the effects of remedies upon it. Thus, some remedies are found to act curatively in one stage rather than another, and others to act curatively alike in several. Among the last is mercury. When chronic inflammation abides long in the stage of mere vascular repletion, mercury will often arrest it there, so that it shall finally cease before it has proceeded to effuse serum or lymph. In those forms of ophthalmia called rheumatic, the special redness of the sclerotic and the vascular zone, and the general redness of the conjunctiva, after they have long existed together with their characteristic pain, and have long refused to yield to other remedies, have often gradually yielded to mercury, producing salivation.

And in the same forms of inflammation, now advanced to a more onward stage, the hazy cornea, and the turbid aqueous humour, and the discoloured iris, and the irregular pupil, have been gradually cleared up, and been restored, as

the mouth has been gradually made sore by mercury.

But, in the whole range of diseases, the cases are few in which we can gain sensible demonstration of what is going on within the part. Most frequently we are obliged to reason and to calculate and learn from the manner in which function and sensation are affected, what is the part, and what is the disease, and what is the fittest remedy. Yet, without doubt, the little that falls within the reach of the senses, is the best and readiest help to much more that lies beyond them. Thus the visible forms of chronic inflammation, and their visible cure by mercury, exemplified in the eye, suggest what many forms of unseen chronic inflammation are likely to be, and the probable success of mercury for their cure. Accordingly mercury has come to have a frequent place among the remedies of such inflammation when it is believed to exist.

Still to draw our instances from the eye, vision is often lost, or impaired in various degrees, where we do not discern the actual disease, but believe it to be seated in the optic nerve or the retina, and where, from circumstances attending the loss or impairment of vision, we conceive the disease of the retina or optic nerve to be chronic inflammation, or some of its effects. This notion of the disease at once points out the remedy. And it is remarkable in what numerous instances of

amaurosis vision has been gradually restored by mercury pushed to salivation. The success of the remedy plainly denotes the nature of the disease. In many such affections medicine would be absolutely without resource but for mercury.

But to draw our instances from other organs, there are numerous affections of the limbs in which sensation or voluntary motion, or both, are perverted, impaired, or lost ; and numerous mental affections, in which intellect and moral perception are disturbed, or injured, or annulled. The disease, without doubt, is in the spinal marrow, or in the brain. But of what kind is it? Often the history of the patient will tell us something, his habits, his mode of living, or injuries, accidents, and by-gone complaints which have befallen him ; and often the history of his symptoms will tell us more,—how they began, and how they have proceeded hitherto, and what has done good, and what has done harm ; and all these together may point strongly to chronic inflammation. And happily this chronic inflammation may be still lingering in some stage short of that disorganisation which is destructive and irreparable.

Upon these fair presumptions the treatment has been instituted, and mercury has been employed among other remedies, and salivation has been gradually produced and long maintained ;

whereupon the symptoms have gradually cleared up and finally disappeared altogether.

Many of the most satisfactory examples which I have seen of the curative powers of mercury, have been furnished by these forms of disease, in which I considered that I had chronic inflammation to deal with.

But let me be very careful of misleading you. Very many have been the instances in which I have seen mercury utterly fail to exercise any curative impression whatever, where the presumption of chronic inflammation has been as strong as possible, and where the fact has been put beyond a doubt by examination after death.

The truth is, that chronic inflammation, in organs remote from observation, often proceeds so much by degrees and so covertly, as to give no intelligible notice of itself, until it has already reached results which are irreparable. The steps and stages of chronic inflammation are not to be distinctly traced and calculated except by the eye.

Again, very many have been the instances in which the treatment by mercury and other antiphlogistic remedies have not succeeded for the cure of supposed chronic inflammation; because, in fact, no such inflammation has existed, but some other disease, in its own nature incurable.

Now when a disease is thus taken and treated for inflammation, and turns out to be no such



thing, and taken and treated for curable, and turns out to be incurable, there is ignorance no doubt on our part, or there is mistake, and some may think there is blame. But it is such ignorance as must be, such mistake as cannot be helped, such blame as the best and wisest of us all have no power of escaping. From the nature of things it cannot be otherwise.

There were at the same time under my care in the hospital, two men completely paraplegic. Both had lost sensation and voluntary motion of the lower extremities by slow degrees. This was all that could be said of their symptoms. In neither of them was there any fever, or vascular excitement or pain. From the account which they gave of themselves, there was reason to believe that the disease of both had originated in exposure to cold some weeks ago, and that that disease was chronic inflammation of the spinal marrow, or its coverings.

Both were treated by the same remedies, by counter-irritants to the spine, and by mercury. In both, salivation was gradually induced, and was long maintained.

Well, what were the results? One patient, as soon as the mercury gave notice of its specific effect upon the constitution, showed an earnest of improvement. Sensation and the power of motion returned very slowly, but at last very completely, to his limbs, and after three months he walked

out of the hospital well. The other never showed the least sign of improvement, and, after lingering in the hospital for many months, he died. Upon examination a small scrofulous tumour was found growing from the theca, and pressing upon the spinal marrow.

Now I can fancy an uninformed looker-on coming to a very unjust judgment upon these results, and giving a very unjust award of praise and blame accordingly; applauding what he might take for a clear insight into the nature of the disease in the one case, and the consequent success of its treatment, and censuring what he might take for ignorance and mistake, and consequent failure, in the other. But there is neither praise nor blame in the matter. You cannot be sure of the success of your remedy, while you are still uncertain of the nature of the disease, as you must be here.

The diagnosis of disease is often easy, often difficult, and often impossible. Why it is so, would be a most interesting and profitable inquiry; but a very large one too, much too large for us at present. But I must just touch upon one part of it (the part which here concerns us), and try to show how it happens that the diagnosis of disease is often absolutely impossible.

All diseases are known by their symptoms; and all symptoms may be regarded under two general aspects, viz. those which denote the dis-

ease in its essence, and those which denote the disease in its seat. Thus, inflammation has symptoms arising out of itself as such, which denote it to be an inflammation; and it has symptoms arising out of the disturbed functions of the organ it occupies, which denote what that organ is, as the brain, the lungs, the heart, the stomach.

Now, in proportion as inflammation is more acute and rapid, both orders of symptoms are more express and prominent, and the diagnosis both of what is the disease, and of what is its seat, is equally certain. But in proportion as inflammation is more chronic and tardy, the symptoms which flow from its essence are less and less marked; they may even become indiscernible, so that they cannot be said to exist at all. And then the diagnosis of what the disease is, becomes necessarily more and more obscure, and may be so obscure as to amount to no diagnosis at all. Yet in the mean time the symptoms which flow from the part are distinct enough, and become more and more distinct from week to week; and thus, while a very obscure diagnosis of the nature of the disease is all that can be obtained, or no diagnosis at all, the diagnosis of its seat is clear and undoubted from first to last.

There is, then, a hard necessity in the case, and there is no help for us; while of the organ affected we are certain, at the nature of the disease

we can only guess. Yet in so guessing, surely we ought to be led by our hopes, and by the possibilities of doing good. We should always presume the disease to be curable until its own nature prove it otherwise.

But when the question is of chronic disease affecting the structure of parts, to presume that it is curable is tantamount to presuming that it is simply inflammatory; and this at once suggests the use of mercury.

Now, I once heard an old experienced physician say, that most of the obscure diseases were cured by mercury. This is as much as to say that most of the obscure diseases are of the nature of chronic inflammation. It would perhaps be nearer the truth to say "many," instead of "most," and this would still leave sufficient encouragement for the employment of mercury.

But one point yet remains to be well considered. I have spoken already, as we went along, of mercury so given in one case as to produce salivation rapidly or at once, and so given in another case, as to produce salivation slowly or gradually. But, if salivation do but take place, does it matter much to the cure whether it arise sooner or later? Yes! indeed does it. And this is the very point now to be considered. It is as important as any to be found in the whole of practical medicine.

Mercury, as a remedy for inflammation, requires

to be administered with as careful an adaptation of its dose to the exigencies of each particular case, as bleeding does of its mode and quantity. The principle of practice is this, to measure the force and rate of the counteractive impression to be produced, whether by bloodletting or by mercury, by the force and rate of the disease. If you have an inflammation of the greatest force and greatest rapidity to deal with, and organic changes are taking place from hour to hour, and bloodletting is to be your remedy, its mode must be by venæsection, and its quantity must be of large amount; and it may need to be repeated again and again, and at short intervals. The bloodletting must make itself felt with a force and a rapidity which shall exceed the force and rapidity of the disease, otherwise it is no remedy at all. So, too, in the same conditions of inflammation, if mercury is to be your remedy, its dose must be large, and repeated. Calomel must be given in the dose of half a scruple at once, and again after six or eight hours. The patient's life may depend upon his being salivated within a couple of days. The mercury, like the bloodletting, must make itself felt, by its specific effects, more forcibly and more rapidly than the disease, otherwise it will fail of its counteractive operation.

But if the inflammation be of little force, and slow in its progress, and its organic changes such



as are not visible from hour to hour, or even from day to day, but rather from week to week, then if your remedy be bloodletting, its mode must not be by venæsection, but by cupping or leeches, and its quantity must be small, and, if it be repeated, it must be at more distant intervals. For now it is essential to the cure that the counter-impression of the bloodletting should be kept down to the small measure of power, and the slow rate of progress, belonging to the inflammation; otherwise it is no remedy at all. And in like manner, if mercury be your remedy, it must now be given more sparingly and less frequently. The object is to produce salivation, not as soon as possible, but slowly and without surprise or violence. A grain or two of calomel once or twice a day, or even some milder preparation of mercury, will bring its specific effects to bear upon the disease after many days, but still remedially.

Upon the principle here laid down and illustrated by the use of bleeding, as it is the important principle which is also to guide us when we employ mercury for the cure of inflammation, let me say a few words more.

Partly, I think, this principle would be at once accepted as true, and partly it would seem questionable at first sight, and would need experience to confirm it. That the bloodletting must be copious, and of the most general kind, and that the doses of mercury must be large, which are

to be counteraetive of severe and rapid inflammation, may be easily conceived. It looks as if it necessarily must be so. But that the bloodletting *must be* scanty, and of that kind which is local, and that the doses of mercury must be small, which are to be counteraetive of the inflammation that proceeds slowly and is of small power, would hardly be expected. One should rather be disposed to argue *à fortiori*, that copious bloodletting by venæsection and large doses of mercury, being able to cure inflammation of great force and rapidity, could not fail to obtain an easy mastery over that which is of little force, and proceeds tardily: that small bleedings and small doses might now be enough, and yet that large ones would do the business more summarily and at once. But experience comes in to rectify such calculations, teaching us this general fact, that small bleedings and small doses of mercury are undeniably curative in forms of inflammation, where large bleedings and large doses exercise no remedial power whatever. A large venæsection will (as it were) leap over the disease without touching it, and afterwards a few leeches will bring it safely and gradually to an end. A rapid salivation will pass by the disease, and leave it unaltered. But when this salivation has been allowed to wear itself out, and the constitution been left to forget (as it were) the impression, and to recover from it, then the remedy being resumed on other terms,

and administered in very small and very cautious doses, has wrought, in process of time, an easy and effectual cure.\*

\* I think it well here to subjoin in a note certain remarks upon conditions favourable or essential to the remedial effects of mercury, which I made many years ago, when an opportunity had been recently afforded me of watching its operation upon a pretty extensive scale : —

“As in regard to the various bowel complaints, so in regard to the various nervous disorders, the condition most essential to the success of the remedy was unquestionably this, that the force and rate of its impression should be in proportion to the force and rate of the disease. And the chief object of our care was to preserve that proportion.

“Thus, where the disease was less severe, and was slow in its progress, salivation (without reference to its degree) was to be procured gradually; where the disease was more severe and rapid in its progress, salivation (without reference to its degree) was to be procured at once. Headache and vertigo, which had come on tardily, and had abided many weeks, without any perceptible excitement of the circulation, were to be made to yield under the slow and alterative influence of mercury, which the constitution could bear without injury. Headache and vertigo, which had been sudden in their accession, were accompanied with excitement of the circulation, and already seemed to threaten something beyond themselves, as convulsion, or delirium, or frenzy, were to be at once mastered by such a sudden and powerful impression of the remedy as the constitution would severely feel. Hence the quantity of the remedy was continually varied, according to the exigencies of particular cases. For some we prescribed one grain or two grains of calomel, with a small quantity of opium, once or twice in twenty-four hours, and thus suc-

ceeded in procuring relief after the lapse of a week or ten days ; doing no harm in the mean time, to the general health and sensations of the patient. For others, we prescribed five, or ten, or even twenty grains of calomel, with proportionate quantities of opium, once, or even twice, in twenty-four hours ; and thus succeeded in dissipating the symptoms at once, and in reseuing life at the expense of some present injury to the constitution." — *An Account of the Disease lately prevalent at the General Penitentiary*, p. 113. ed. 1825.

## LECTURE XV.

USE OF MERCURY IN THE TREATMENT OF RHEUMATIC ENDOCARDITIS AND PERICARDITIS. — EVERY GREAT ADVANCE OF CLINICAL AND PATHOLOGICAL KNOWLEDGE REQUIRES THAT OLD REMEDIES SHOULD UNDERGO THE TRIAL OF NEW EXPERIMENTS. — IN ENDOCARDITIS THE REMEDIAL POWER OF MERCURY SHOWN, NOT SO MUCH BY THE RESULT OF SINGLE CASES, AS BY THE COMPARATIVE RESULTS OF MANY WHICH HAVE, AND OF MANY WHICH HAVE NOT, BEEN TREATED BY IT. — IN PERICARDITIS ITS REMEDIAL POWER MAY BE APPRECIATED IN INDIVIDUAL CASES. — THE RELATION WHICH THE CESSATION OF THE EXOCARDIAL MURMUR BEARS TO THE CESSATION OF THE DISEASE. — THE POWER OF MERCURY TO PROCURE THE CESSATION OF THE MURMUR, EARLY OR LATE, ACCORDING TO CIRCUMSTANCES. — EARLY SALIVATION MOST STRIKINGLY CURATIVE. — LATE SALIVATION NOT WITHOUT BENEFIT. — COMPARISON OF SOME GENERAL RESULTS.

WHILE we were considering the treatment of rheumatic inflammations of the heart at each several step, and with the recommendation of each several remedy, I kept your attention alive to another remedy, which was yet to be noticed. This is mercury. Bleeding, in its various forms, must be used, I told you, for its own direct curative power over the disease; but so used,



that it should not hinder the curative power of mercury. Opium, I told you, must be employed to pacify the nervous system ; but *so* employed, moreover, that it should at the same time aid the curative power of mercury. Then the curative power of mercury over inflammation in general was considered ; and now we come at last to its curative power over rheumatic inflammations of the heart, and I will endeavour to give you as fair an estimate of it as I can.

Many years ago, in certain Essays which I published "On some Diseases of the Heart \*," I ventured to insist, that mercury pushed to salivation was indispensable to the cure of pericarditis. And at that time I did not insist upon less than the results of my experience, carefully considered, seemed to warrant. But then my knowledge of the disease was very defective ; defective especially in respect of its diagnosis during the life of the patient ; and so was then every body's knowledge. For now I plainly perceive that the majority of the cases in which I then believed myself treating an inflammation of the pericardium, were, in fact, cases of endocarditis.

As diseases are better understood, and we possess surer signs for discerning their seat and progress, and events, the records of past experience become obsolete, and so a necessity arises

\* Med. Gazette, vol. iii.

for a new course of clinical observations. Even each man's own stock of observations, if in his time knowledge has made a great step in advance, observations which he has carefully kept in mind, and which have served him to draw conclusions from, he may, after all, have good reason to distrust.

What an amazing difference there appears in the objects of nature around us, according to the point of view from which we regard them! When we stand on the right spot for taking in the whole prospect, we then see what before we could not see at all, or we then see clearly what before we only caught a glimpse of, from some less commanding position.

Thus, the point of view from which diseases of the heart are now regarded, discloses so many new things, and puts so many old things in a much clearer light, that I distrust the results of my former experience, and feel the need of submitting all my practice, and the use of all my remedies, to the test of my own more recent observation. I feel that the use of mercury, especially, requires to be brought to this test.

Formerly I gave mercury, or designed to give it, in every case of pericarditis, and sought to procure salivation, which I deemed indispensable to the cure. And thus thinking to give it in every case of pericarditis, I probably gave it also in every case of endocarditis, not being then able

to distinguish one from the other. But now, when I look back, I plainly perceive (whatever I might then have thought), that the impossibility of then forming a diagnosis between the two stood in the way of my gaining a sure proof of the benefit of mercury in either. The diagnosis however, between the two, being now plain and obvious, I will seek the evidence of the use of mercury in the treatment of each separately.

And first, of endocarditis.

In my clinical records, I find some cases of endocarditis in which bleeding and common antiphlogistic remedies alone were employed, and not a grain of mercury was given; and yet all the symptoms referable to the heart, the pain, the palpitation, the dyspnœa, and the endocardial murmur itself, entirely ceased. In short, there was all the evidence that could be required of a perfect cure. But I find no cases in which mercury alone was given, and not a drop of blood was taken, and no other antiphlogistic remedy was employed, and yet perfect reparation followed. While, then, I have facts which claim an independent remedial power for bloodletting, I have none which claim the same for mercury.

Again, I find some cases of endocarditis in which bleeding was used, and mercury given conjointly, and the evidence of cure was satisfactory; but the mercury produced no salivation. Here one cannot tell what share the mercury had

in procuring the result ; one cannot even be sure that it had any share at all.

Again, I find some cases in which bleeding and mercury were employed conjointly, and salivation quickly followed, and every vestige of the disease was swept away at once. But here, the modes in which the remedies took effect, and the symptoms ceased, were such, that no opinion could be formed how much of the cure was due to the bleeding, and how much, if any part of it at all, was due to the mercury.

Again, I find some cases in which bleeding and mercury were employed conjointly, and salivation followed, but it was slow to arrive. And reparation was complete in the end, but it was after a long time. Here the manner and gradations by which the disease declined appeared to correspond with the sensible operations of the remedies, and to denote, with seeming exactness, the curative influence belonging to each. The bleeding was practised, whereupon vascular action immediately abated much of its force, and pain, and palpitation, and dyspnœa, immediately went away, but the endocardial murmur remained. Mercury, too, was given from the first, and day after day it was still given, yet there was no salivation. At length, however, salivation arose, whereupon the endocardial murmur ceased.

These several orders of cases exhibit fair specimens of the sort of difficulties which are apt to



obstruct us, when we seek to analyse the effects of medicines. We may be well satisfied with the general results of treatment; but when we betake ourselves to calculate the separate value of the means by which these results are brought to pass, we may not be able punctually to determine what it is.

If I were called upon to bring sure proof of the remedial power of mercury in endocarditis, the last, perhaps, are the only cases to which I should be allowed to appeal; and these claim for it (what I have explained to be) a reparatory, not an anti-phlogistic power. They do not satisfy us that it had any thing to do in counteracting the progress of the inflammation. They only show us that it came in aid of nature in restoring the endocardium to its integrity, after the inflammation had ceased.

But although in endocarditis I cannot produce proof, beyond cavil and exception, of more than a reparatory power belonging to mercury, yet my impression is so strong that it does exercise, moreover, a power purely antiphlogistic and auxiliary to bloodletting, that I dare not omit to give it in every case as soon as I have ascertained the nature of the disease, and to press it to salivation.

Analogy favours the belief of its being anti-phlogistic as well as reparatory in inflammation of the endocardium. It is obviously so in inflam-



mation of some other structures — of the iris, for instance.

At all events, the *reparatory* power of mercury in endocarditis is tolerably certain, and the earlier we employ it, the more likely is this reparatory power to come into effectual exercise. Mercury being administered coincidently with the use of bloodletting and other remedies, though it may possibly not add any antiphlogistic power of its own to theirs, yet in the mean time it will be making its way in the constitution, and will be ready to further the work of reparation when the inflammation has passed away.

Thus, when I take my own experience in detail, and examine the results of treatment case by case, I cannot pretend to have found a certain proof that mercury is an indispensable remedy to the cure of endocarditis. But taking my experience in the mass, I still fear to omit its employment in any case of endocarditis with which I have to do.

No doubt, by bleeding and other ordinary antiphlogistic remedies, præcordial pain and palpitation, and even the endocardial murmur, have been known entirely to disappear, when mercury has either not been used at all, or being used has not produced salivation.

But against the particular result of these cases, as seeming to exclude mercury, I would set the general result of my entire experience, as seeming (to me at least) to recommend it. Since the time

that auscultation has disclosed the sure diagnosis of this disease, it has not in a single instance proved fatal under my care. But M. Bouillaud, to whom the world is greatly indebted for bringing its diagnosis to perfection, records numerous instances in which endocarditis terminated fatally under his management.

Now M. Bouillaud's treatment of endocarditis has always been vigorously antiphlogistic. He has employed large and repeated bleedings, and all other remedies calculated to control inflammation, except mercury. Mercury he never used.

My treatment of endocarditis, on the other hand, has not been vigorously antiphlogistic. I have seldom employed venæsection at all, and never largely. But mercury has been among my remedies in almost every case.

From this comparison the conclusion is irresistible, that mercury has the power of doing something more in counteraction of inflammation of the endocardium than venæsection and other antiphlogistic remedies, and that upon this something being done the life of the patient often depends.

What this something is we can only conjecture from analogy, but yet with great probability of truth. Undoubtedly it consists either in controlling the disease, or in restoring the conditions of health, or in both. For these are the only

ways, as far as I know, in which mercury exercises its curative influence.

Experience thus testifies to the broad fact of mercury being instrumental *to the saving of life* in endocarditis. And unquestionably so it may be, and yet not carry its curative powers to perfection in every case. From the records already given, it will be seen, how many subjects of endocarditis are brought through a formidable disease by antiphlogistic and mercurial treatment, and survive and are safe; but still they have the endocardial murmur, and never lose it. Here the mercury has indeed fallen short of restoring perfect integrity of structure, but it has had its share in saving life nevertheless. Some unevenness, some thickening, puckering, or shortening of a valve, or a bead of lymph upon it; this may be all that remains. But this is enough to produce an eddy of the blood, and the eddy to produce an audible murmur, and thus the heart ever afterwards passes for unsound. Yet any other internal organ which, from being inflamed, should come so near reparation, would pass for sound, because we should not have the means of knowing it to be otherwise.

In those diseases where the remedial power of mercury is least questionable, reparation is apt to take place, leaving behind a mark or a scar. Thus, the pupil often remains slightly irregular after the cure of iritis, and clefts remain in the tonsils after the cure of an ulcer. No evil, how-

ever, results. But the merest scar within the heart is, from the nature of its functions, a grave matter in its ultimate effects. Still it is no disparagement of the remedial powers of mercury here, more than elsewhere.

In conclusion, the simple fact of the much larger proportion of cases in which life is saved where mercury is used than where it is not, is a plain paramount recommendation of it as a remedy for endocarditis, which all can see and understand.

We come next to the use of mercury in pericarditis. In every one of my eighteen cases, complicated and uncomplicated, mercury was employed. But then it was employed conjointly with other remedies; so that my experience does not furnish me with a single case from which I should be allowed to infer conclusively the curative effect of mercury without the aid of other remedies, or the curative effect of other remedies without the aid of mercury. Still I should be sorry to omit the use of either in any case of pericarditis with which I had to do. But the question at present is only concerning mercury. And, allowing bleeding and common antiphlogistic measures to be needful, and even indispensable, I am fully persuaded that let them do all which they can do, mercury can do something more; something towards saving life and inducing reparation, which nothing else can do, or nothing else can do so well. Of this there is as satisfactory evidence as we have of



most points in practical medicine which are thought settled.

Before I come to a closer examination of this evidence, I would mention one remarkable fact. Of the eighteen cases of pericarditis, which are the subject of our present commentary, and were treated by mercury, some were brought under its sensible influence very largely, and some very slightly, but all in a certain degree except two. In these two cases, though mercury was given in large quantity and for a long period, yet was there no sensible ptyalism, no factor of the breath, no complaint of soreness of the gums. In these two cases, while mercury was pushed thus strenuously, other remedies were vigorously employed; and, moreover, in these two cases every conceivable circumstance was present which could promise success to medical treatment. The subjects were healthy subjects and in the prime of life. The disease (there was reason to believe) was detected as soon as it arose. Not a moment was lost in the application of remedies. They were venæsection and cupping and leeches and blisters and opium and, from first to last, mercury. But the mercury, as I said, did not produce the peculiar effects of mercury in the slightest appreciable degree.

Now of my eighteen cases of pericarditis I lost three; and these were two of them.

But let us see whether we cannot get a little



nearer insight into what mereury does in pericarditis. And let me premise that in pericarditis the symptoms, both auscultatory and non-auscultatory, give a more exact intelligence of what is going on from day to day than they do in endocarditis; and we are able to follow the steps of the disease, and to appreciate the effects of remedies, more surely and satisfactorily. And, as of other remedies, so of mereury.

Now my strong impression is, that pericarditis, of that extent and degree which it generally reaches in acute rheumatism, though it be treated by the best remedies and in the most opportune and efficient manner, is never so completely cured that the parts regain their perfect integrity of structure; in short, that in the most favourable event the pericardium almost always adheres. Medical treatment saves life, but it rarely prevents the adhesion.

But then, has the medical treatment of pericarditis no further aim than barely to save life? Has it only to provide that the exocordial murmur should cease, and the patient continue to live on any terms? Yes! it seeks much more than this. It has, indeed, first to save life; and it has further to provide that the life which it saves shall go on with the least possible hinderance, and suffer the least possible abridgment of its natural duration. And this it does when it arrests the progress of the disease, and, moreover, provides that the re-

paration (which I fear is rarely perfect) should take place with the least possible degree of imperfection; that is, that the folds of the pericardium should be brought together again, and should permanently adhere with the least possible quantity of intervening lymph.

When this is brought to pass, remedics have done their best. But, for the sake of being able to judge how far remedies are actually doing their best in particular cases, we should bear in mind, that the final cessation of the exocardial murmur probably denotes the adhesion of the pericardium, and that the sooner this occurs the less accumulation of the products of inflammation must have preceded it, and the more perfect, or, rather, the less imperfect, is reparation likely to be in the end.

Thus it is a great thing for the exocardial murmur to begin and cease in a week. I can refer to three cases only, in which I am sure that such was the fact. And there was not one of them in which the patient was not first salivated. In two other cases the exocardial murmur ceased, in one on the sixth, and in the other on the eighth day, after they were brought under my observation and treatment, and in both, the patients were first salivated; but in them I had no certain knowledge how long the murmur had been audible before admission into the hospital. In these several cases not only did the murmur cease, but the whole business of medical treatment was accomplished.

With the cessation of the murmur life was apparently safe, and convalescence followed rapidly.

Here mercury seemed to me to display its highest antiphlogistic power. But, if others doubt, let it be a question for future observation, whether, where the murmur of pericarditis rapidly ceases and the danger rapidly disappears and convalescence rapidly follows, salivation is or is not a preceding condition, in all, or in the vast majority of cases. There is no question of practical medicine more important to have rightly settled.

But I am anxious not to be misunderstood in this matter, or thought to state either more or less than I mean. My experience (as far as it goes) tells me, that whenever the exocardial murmur has ceased early, salivation has first taken place. But it does not affirm the converse, viz. that, wherever salivation has taken place early, the exocardial murmur has ceased early. These are very different things. The early cure *may not* take place without the early salivation, but the early salivation *may* take place without the early cure.

When, therefore, in after times, you come to treat this disease, should you succeed in bringing your patients speedily under the influence of mercury, and find in one case a speedy cessation of the murmur and a speedy arrest of all the more formidable symptoms, and find in another case the murmur still remain and still abide for a long time afterwards, and other formidable symptoms,

mitigated indeed, but more slow to disappear, do not say that I have misrepresented the power of the remedy. All this is according to my experience; for turning to my records of cases I find six in which salivation was rapidly produced, with the following different results as to the period at which the murmur ceased:—

Cases.		Days.		Days.
1st.	Salivation produced in	1	Murmur ceased. in	4.
2d.	—	2	—	7.
3d.	—	3	—	4.
4th.	—	4	—	28.
5th.	—	5	—	14.
6th.	—	5	—	25.

Of these six cases mercury was most eminently remedial in the three first, less eminently but still remedial in the three last. The cessation of the exocardial murmur at such widely different periods after salivation appeared, is satisfactorily accounted for by the circumstances of the different cases.

In the three first cases mercury was given as soon as the murmur was audible, and salivation followed in one, two, and in three days; and the entire duration of the murmur was four days in the first, seven days in the second, and four days in the third case. Here there was reason to believe that the murmur denoted the commencement of the disease. The patients were under observation before the murmur arose. Prior to it there was no other symptom referable to the heart;



and thus the remedy had an equal start with the disease. It was ready to sway and counteract the first inflammatory movement, and still to keep it under day by day, and ultimately to withhold it from terminating in more than a scanty effusion of lymph within the pericardium. Thus, when the inflammation ceased (as it did) quickly, reparation was short and easy; there was little effused, and little to be absorbed. The folds of the pericardium soon came together again, and were soon restored to their previous state, or soon adhered.

In the three last cases, too, mercury was given as soon as the murmur was audible, and salivation followed in four, in five, and in five days. But the entire duration of the murmur was twenty-eight and fourteen, and twenty-five days in each case respectively. Here there was reason to believe that the murmur did not denote the commencement of the disease. Prior to it for some days, and before the patients came under our observation, there were other symptoms referable to the heart,—severe pain and anguish and inordinate impulse. And thus the disease had the start of the remedy. The inflammation was at first, and still for a while, unchecked and uncounteracted, and ultimately was not withheld from terminating in a large effusion of lymph and serum within the pericardium; and thus, when it ceased, reparation became long and difficult. There was much



effused and much to be absorbed. The folds of the pericardium were slow to come together again, and slow to adhere. When acute inflammation has existed only for a day or two, and has done the mischief only of a day or two, and the remedy has been brought to bear upon it rapidly, fully, and successfully, then the changes from bad to good may be plain, palpable, and at once. The symptoms of the disease may be swept away, and the mischief done in a day or two, may be undone in a day or two, and all may soon be well. But when inflammation has gone on for a week or ten days, and has done the mischief of a week or ten days, then, though it may still be within the possibility of cure, it cannot be brought to yield instantaneously to the curative impression of any remedy. The changes from bad to good will not be discernible at once. What it has taken a week or ten days to do, it will take at least a week or ten days to undo. All may be well in the end, but all cannot be well speedily.

Now in these last cases, considering the long interval that elapsed between the appearance of the ptyalism and the cessation of the murmur, and considering the fact that other remedies were employed together with mercury, I do not pretend to assign to mercury the exact share it had in procuring the result, and I can well pardon any man, who is not satisfied from my mere statement, that it had any share at all. But all who wit-

nessed the cases were satisfied, from their own observation, that it had a material share. We were all struck by this remarkable circumstance, that the whole terror of the disease was compressed within the few days which *preceded* the salivation.

Præcordial pain, and anguish, and fluttering, and gasping for breath, and pallor, and delirium, and nervous exhaustion, and threatened syneope, all in their extreme degrees, made death the apprehension of almost every hour, for four days and nights in one case, and for five days and nights in the two others. But though the murmur still continued in one case twenty-four days, in another nine days, and in another twenty days after the salivation, yet no sooner did it take place than the terror of the disease was gone. Henceforth the cases still needed anxious watching, and still needed careful treatment. But it was treatment of a different kind, and upon different terms. It was such as, instead of continually applying and pressing remedies to counteract progressive inflammation, kept them in reserve, and brought them to bear upon this or that distressful symptom as it happened to arise. The inflammation seemed gone, but its effects remained; and both the constitution and the heart itself required some helps of medicine, that they might be enabled to sustain them until such reparation as was possible should be finally accomplished.

Such are the remedial effects of mercury, when it enters into the treatment of pericarditis, and produces salivation *rapidly*.

But mercury may enter into the treatment of pericarditis, and produce salivation *slowly*, even very *slowly*. It so happened in five of the cases which fell under my care. In one it took eight days, in two eleven, and in two, thirteen days to procure ptyalism or factor of the breath or soreness of the gums.

Now in all these cases, bleeding and other antiphlogistic remedies and opium were meanwhile employed from day to day, and with good effect. They kept down vascular and nervous excitement, they assuaged pain, they abated palpitation; and in the end the exocardial murmur ceased, and life was saved.

These cases, perhaps, you would throw aside at once, and think it a foolish fancy to be searching into them for proof of any remedial power of mercury. Perhaps you would think that to those other means of acknowledged power and efficacy, which did manifest service from day to day, must be justly ascribed the whole credit of finally saving life and inducing reparation, and that to mercury, which produced no sensible effect until eight, or eleven, or thirteen days were past, cannot reasonably be due the smallest share in the result.

Nevertheless, I must profess my strong per-

suasion that mereury had a share, and an important one, in the result.

In all the five cases common antiphlogistic remedies were fairly and fully employed; and, when no salivation appeared after many days and it became more and more doubtful whether it would ever take place, they were used with the more earnest purpose of making them do all they could do, as if the whole cure depended upon them, and as if there was no other remedy in reserve, no mereury, which might yet come into operation at last, and complete the work which they had left imperfect. Now what happened? Not in a single instance of all the five was there the best and surest evidence of inflammation arrested, and reparation begun, until mereury, though late, had produced its specific effect. Not in a single instance did the exocardial murmur cease to be audible, until salivation appeared.

Recollect the common antiphlogistic remedies had already had in one case eight days, in two cases eleven days, and in two cases thirteen days allowed them, to do all that they could do alone. And it is strange, indeed, that in some one or two at least of these cases they should not have procured the cessation of the murmur, if they alone were capable of procuring it, before the salivation arrived.

This is a fact worth noticing; but it is merely a negative fact, and cannot be pressed to bear



testimony to the comparative efficacy of this or that remedy. As to the exoeardial murmur, though it ceased at last in all these cases, yet it continued in every one of them for a long time after salivation had taken place, in some for several days, and in some for more than a week. But, prior to its cessation, there were signs which gave us assurance of inflammation being brought to an end, and of life being saved; and these were so coupled with the occurrence of salivation, that it would have been unreasonable to doubt of mercury being mainly instrumental to the result.

I will relate, then, summarily, the plan of treatment, and the progress of recovery in these cases, marking the changes consequent upon the impression of the several remedies, and of mercury among the rest.

At the beginning, every thing seemed favourable to the cure. The treatment commenced with the administration of mercury in the way best calculated to insure its specific effect, and proceeded, without remission of its use, day by day. And from the first, also, and day by day, venæsection, or cupping, or leeches, blisters, and opium, were employed, according as indications required them. And these fulfilled their immediate purposes. Pain, and anguish, and dyspnœa, and palpitation, were relieved. Yet there was no salivation. But pain, anguish, dyspnœa, palpitation, all, or several



of them, returned ; and they were again relieved by some, or by several of the same remedies. Yet still there was no salivation. And again, in like manner, the same symptoms returned, and again were relieved by the same remedies. Yet even still there was no salivation.

But after several times thus going and coming, the symptoms became more and more modified by weakness, and at last fearfully so. And after several times thus giving relief, the remedies gave relief less and less, and at last not at all. Thus medicine seemed at the end of its resources, and the patient sure to die.

Now at the commencement of the disease, when mercury had been given for two or three days, and no salivation appeared, other remedies (it has been said) were pressed more earnestly and vigorously, from an apprehension that our sole dependence must rest upon them. Nevertheless, mercury continued to be given all along, and more largely as the constitution was found more reluctant to accept its influence, and more largely still as other remedies seemed losing their power. Then, in addition, to as much calomel, united with opium, as the stomach and bowels would bear, a drachm of strong mercurial ointment was rubbed in three or four times a day.

But after long delay, and in our utmost need, salivation came at last — and with the salivation there came a pause of the disease. The patient

whom we expected to die yesterday, was found alive to-day. Had the evil symptoms of yesterday been augmented in the least degree, he must have died. It seemed hardly possible ; but they remained just as they were, and he was alive still. The disease had almost touched upon dissolution, and *there* it paused.

But could this mere pause, the patient being yet in such extremity, be taken for a ground of hope ? Yes ! even for a strong ground of hope, occurring, as it did, under such circumstances, and coincident with salivation. For in these cases, after another day or another night, or sometimes within twenty-four hours, amendment began to follow this pause ; amendment, however, which was rather in the patient's own feelings than in our knowledge. Nevertheless, real amendment. We could not yet calculate the particulars in which he was better : yet he felt himself better. He had less nervous alarm, less starting from sleep, less fear of syncope from accidental movements of the body to this side or that.

To *our observation* there was still no change of *symptoms* immediately referable to the heart. From what we could learn by our percussing, and listening at and feeling the chest, there was the same præcordial dulness, the same exocordial murmur, the same unequal, feeble, fluttering impulse. But there must have been some change in the *actual* conditions of the disease ; and that

change might have amounted to an arrest of the inflammatory movement at least, and a stop of further effusion. We could not tell what it was. The patient had the witness within his own nervous system, and in his own inward consciousness, that a change had taken place, and for the better. And we had witness of the same in those sure outward manifestations, by which the nervous system signified, both waking and sleeping, that it was more at ease.

Thus from the time that the ptyalism appeared, although the exocardial murmur was still audible in one case for several days, in another for more than a week, and in another for more than a fortnight, we began to feel assurance, first that the disease had come to a pause, and then that the patient was further and further from death, and nearer and nearer to a state of safety. And it was pre-eminently the nervous system which began and continued all along to give us this assurance.

But the nervous system was not only the chief witness, it had also now become (if I mistake not) the great agent of the patient's safety and recovery. And, as such, it now demanded our chief care. We had done with addressing remedies to the vascular system. Bleeding had had its effect, and mercury was now happily in the course of its operation. Our business was to soothe, and tranquillise, and comfort the nervous

system. There was little more to do, but there was need of doing it effectually and well. I recollect, in a particular instance, that four hours' sleep, procured by the dextrous use of opium, marked the exact period of the patient's safety, and did the work of a week in furthering his convalescence. He woke with the number of his pulse, and the number of his respirations, greatly reduced, and thenceforth neither of them ever regained an excessive frequency; and, though the exocordial murmur remained for more than a fortnight afterwards, reparation was manifestly going on.

Such was the course of treatment and the progress of recovery in certain cases of pericarditis, where mercury was slow to procure salivation, but procured it at last. I have endeavoured to make myself as intelligible as I could in noting the circumstances which seemed to assign to the different remedies the shares they had in the result. And their shares, in my judgment, may be apportioned thus:—Common antiphlogistic remedies could mitigate, could retard, could do all but effectually stop the inflammation of the pericardium, and set reparation fairly at work. Mercury took up the cure where common antiphlogistic remedies had left it, and came in with its peculiar power and efficacy to complete what they were not able to accomplish. And then inflammation ceased, and reparation began.



I confidently believe that in every one of these cases death would have taken place at an early period, and long before salivation arrived, had not common antiphlogistic remedies been opportunely and vigorously employed. And I as confidently believe that, in every one of these cases, death would have taken place at a later period, had not the remedial power of mercury been still in reserve, and had not salivation arrived at last.

If we would fairly represent the power of any remedy, we should not merely point to its more striking effect in a few cases, but should be at the pains to exhibit truly its ordinary operation in the majority of cases which occur. On this account, I have dwelt the longer upon the operation of mercury in those cases of pericarditis in which it is slow to produce salivation; for such, undoubtedly, are the majority.

These, also, are the cases which seem to me to contain the surest proof of its remedial power. Not that *then* its remedial power is the greatest, but that from circumstances it can *then* be more clearly seen to be remedial; for they show both how far the cure can be advanced by common antiphlogistic remedies, and how much further it can be carried by the help of mercury.

These, moreover, are the cases in which foreign and English practice in the management of pericarditis may be fairly brought into comparison,



and in which it may be seen where and how the one so often fails, and the other is so often successful.

In foreign practice no mercury is used from first to last, but all the power of common antiphlogistic remedies is brought to bear upon the disease ; and thus its symptoms are mitigated or subdued : yet they return again and again, and are again and again mitigated or subdued. And so the patients are kept alive for a week or ten days, and then they die in the great majority of cases.

In English practice mercury is given from first to last. But it is for a long time as if it were not given at all, for it produces no sensible effect. Common antiphlogistic remedies, however, are able again and again to mitigate and subdue symptoms ; and so at the end of a week or ten days the patients are still alive. Yet they are ready to die ; but in the great majority of cases they do not die. Salivation arrives late and seems to save them.

## LECTURE XVI.

OF ENDOCARDITIS, INDEPENDENT OF RHEUMATISM. —  
THE CLINICAL KNOWLEDGE OF ENDOCARDITIS ALTO-  
GETHER A NEW KNOWLEDGE. — THE WAY IN WHICH  
IT WAS OBTAINED SUGGESTS THE WAY IN WHICH  
IT MAY POSSIBLY BE ENLARGED. — CASE OF ACUTE  
ENDOCARDITIS INGRAFTED UPON CHRONIC VALVULAR  
DISEASE. — CASES OF ACUTE ENDOCARDITIS COMBINED  
WITH PERICARDITIS IN A PREVIOUSLY SOUND HEART.  
— CASE OF SUSPECTED ENDOCARDITIS UNDER A MORE  
CHRONIC FORM. — GENERAL REMARKS.

A FEW years ago, acute endocarditis was a disease almost unknown ; yet, in fact, it must have been just as frequent a disease formerly as it is now. Where, then, lay the secret of our former ignorance and of our present knowledge? Let us try to trace it out ; and in so doing, we shall see what time, what instruments, and what happy opportunities, are all needed to perfect the diagnosis of an internal disease.

Morbid anatomy failed to find it out ; because morbid anatomy had not opportunities sufficient of investigating it. People rarely die of endocarditis, while the characters of an acute inflammation are yet traceable in the heart. Before they die, these are commonly obliterated, and

there remain puckerings and thickenings of the membrane, which are, in truth, the marks, not of the disease, but of its imperfect reparation.

Clinical observation passed it by; because clinical observation had not yet learned the use of auscultatory signs in reference to the heart. And when their use was generally understood, it was still the work of much time, and the labour of many minds, to simplify their application, and to understand how a single sign, denoting only one thing absolutely, is involved in several diseases, and helps us to discriminate them; how the endocardial murmur especially, which only implies mechanical injury done to the endocardium, helps us to determine that this injury is in one case the effect of bygone disease, or slow disorganisation, and in another the product of present and progressive inflammation. Now it is the force of concomitant circumstances which stamps their peculiar diagnostic value upon auscultatory signs. And these circumstances clinical observation had still to study, long after it knew the signs themselves, and understood their primary import. The endocardial murmur it had known, and its primary import it had well understood for years before it reached the sure diagnosis of endocarditis, and was made aware of how important a place it occupies among diseases. It was the study of concomitant circumstances which, in this great instance, both perfected our diagnosis, and

enlarged the sphere of our pathological knowledge.

Acute endocarditis may, hereafter, turn out to be of still more frequent occurrence than it is at present known to be. Future clinical experience may find it in frequent alliance with other forms of disease besides rheumatism; and so, teaching us under what circumstances to expect it, may set us on searching for it, and thus may prevent us from overlooking it when it really exists. The facts for clinical observation to fix upon as evidence of acute endocarditis are these: First, an audible endocardial murmur recently declared; secondly, the coincidence of its origin in point of time with such general vascular action as is deemed inflammatory. For my own part, I have, at present, no familiar knowledge of the conditions indicated, except in cases of acute rheumatism.

I have seen, indeed, acute endocarditis when it has not been in alliance with rheumatism, but neither so often, nor under circumstances so marked, as to gain much instruction by seeing it.

In the following case, endocarditis was concealed under a vast complication of disease. The patient was under my care and observation for a week, yet in the mean time I had not the skill to discern it. It was unfolded by dissection after her death.

*December 19. 1837, Harriet Platford, aged 14, came into St. Bartholomew's to die. Her lips*

were livid, her lower extremities œdematous, her pulse small, frequent, feeble, and irregular. She groaned, she coughed, and she gasped for breath. Auscultation found sibilus and erepitation proceeding from every part of both lungs, but the whole chest resonant to percussion. This indicated a diffused bronchitis. It found also an endocardial murmur accompanying the systole of the heart, which was heard chiefly at the apex. This indicated disease at the mitral orifice. The heart's impulse was greatly increased.

This poor girl, young as she was, was a servant of all work in a humble family. No distinct history could be obtained from her of the origin and progress of her complaint. She had been long ill. It was only during the last three weeks that her legs had begun to swell, and cough and dyspnœa had become severely oppressive.

She was kept alive a week. Diuretics were administered. Very small quantities of blood (two ounces) were thrice drawn by cupping from below the scapulæ, and a blister was applied to the sternum ; while she was upheld in the mean time by small quantities of wine. One day she gained a little relief, and lost it the next. She rallied and sank, and rallied and sank again. At length the ear caught no sound, either healthy or morbid, during respiration. All was dull to percussion. She became more and more livid, was insensible, was convulsed, and died.

Dissection found all that was expected, and



*something more.* All organs were eongested with blood. The liver was large and dark. The kidneys had their pelvis and tubular structure stained of a deep red. The mueous lining of the larynx, and trachæa, and bronchi, were deeply livid. The lungs were gorged. The cavity of the abdomen contained two pints, and each cavity of the pleura one pint, of yellow serum. The pericardium was distended with serum; and the endocardium of both sides of the heart was of a deep red colour. The mitral orifice would scarcely admit the passage of one finger, and one process of the mitral valve and the chordæ tendineæ springing from it were ossified. Added to all this, the lining membrane of the left auricle, near its opening into the ventricle, was covered with little granules and beads of lymph, and the same were found upon the mitral valve. (W. 24. 68.)\*

These last appearances contain the evidence of acute inflammation of the endocardium. Yet, in the present state of our knowledge, no human sagacity could have divined its existence during the life of the patient. It probably arose not many days before death. But its own distinctive murmur was merged in the murmur already existing, and derived from the ossification of the mitral valve.

\* The state of the cavities and the muscular structure of the heart is unaccountably left unnoticed in the record of the dissection.

The case, however, was worth relating in some detail, only to show, under what pathological conditions, besides those of acute rheumatism, endocarditis may occur.

It appears, then, that acute inflammation may be ingrafted upon previously existing chronic disease of the endocardium, and that it may arise during the last days of existence, when the vascular system is especially disturbed, and blood is obstructed and retarded in its passage through the lungs and through the heart.

The next case is made up partly of what was communicated to me, and partly of what I myself observed. From what I understood of the nature of the first attack, and the condition in which I found the heart on auscultation a month afterwards, I have no doubt that the disease was acute inflammation of the endocardium, which had its origin in cold and fatigue, operating upon an enfeebled body.

“A. C., æt. 18, had the nettle-rash at Eton about three months ago. He came to London on its partial subsidence, and being very weak, was ordered a course of steel medicines, which he took for five or six weeks. But he still continued weak, and unable to ride on horseback, or take other active exercise, to which he had been accustomed. Four weeks ago he witnessed a cricket match, and stayed on the ground after sunset, and suffered a chill. I saw him on the 29th of August. Tongue furred, pulse 108,

rigors, pain in the legs, stiffness down the back, and a very slight pain at the cartilages of the lower ribs on the left side. Leeches removed this pain, and he was ordered calomel and James' powder and salines. On the third day of my attendance, he was much worse. The pulse rose to 135, and the heart beat with great vehemence. He was bled by venæsection to 30 ounces, and afterwards had leeches every other day to the cardiac region. He was also cupped, and was brought under the influence of mercury, and had three or four blisters."\*

The daily progress of his convalescence is not detailed in the report, which goes on to say,—“For the last six days the patient's condition has been stationary; pulse 98, heart's action heard to a great extent, but *bellows' murmur much* less than it was. He had never had rheumatism, or palpitation, or any disease of the heart before.”

It will be observed that the “bellows' murmur,” the sign most expressly characteristic of the supposed disease, is not noticed until a very late period. This must have been an omission from inadvertence. For when mentioned, it is so as to imply that it had existed for some time previously.

The patient was brought to town on the 27th of September, for my advice and opinion upon the

\* Thus far the report of the case was furnished me by Mr. Woakes, of Luton, Bedfordshire.

state of his disease. He had a peculiar aspect of distress. His complexion was pale, with a slight flush. His breathing was hurried, and the more so upon the slightest movement. His pulse was very feeble, and very small, and frequent. He had some small purpurous spots on one leg, just above the instep. The lower half of the præcordial region was dull to percussion, and the impulse of the heart was felt far beyond the apex. Being examined in the erect posture, a loud endocardial murmur was heard in every part of the præcordial region, equally at the apex and the base of the heart. At the base the murmur became double, and it was continued, with hardly any abatement of its intensity, along the subclavians and carotids. Being examined in the supine posture, the murmur, as such, was lost to the ear, both in the heart and in the bloodvessels, but still a sound was audible in both, which was different from the natural and healthy sound. The left side of the chest was altogether resonant to percussion, except, as before remarked, in a certain portion of the præcordial region; and a healthy respiratory murmur was given out by every part of the left lung. But the right side was dull to percussion all round, below the level of the angle of the scapula and of the mamma; and it was doubtful whether, throughout this space, there was any audible respiratory murmur. He lay best on the right side.

At the time I saw the patient, and made this



examination, a month had just elapsed since the first attack. What I found gave sufficient evidence of injury done to the mitral and aortic valves, and of fluid effused into the cavity of the right pleura. From a certain jerk accompanying the systole of the ventricles, I suspected, moreover, adhesion of the pericardium.

Five months afterwards, in the following February, he was brought to town, and placed under my care. His body was extensively anasarcaous, and the cavity of the abdomen and of each pleura contained some fluid. He was deeply jaundiced. His liver was felt lower than the navel, and reaching across the abdomen from the right to the left hypochondrium. His jugular veins were full and prominent, and the whole venous system was loaded with blood. The great force and extent of the heart's impulse, and the large space of præcordial dulness, sufficiently declared the left ventricle in a state of hypertrophy; and a loud endocardial murmur audible every where in the præcordial region, and beyond it in front of the chest, while it was carried along the aorta and the carotids, left no doubt of valvular injury both at the aortic and the mitral orifices.

By medical treatment the anasarca was dissipated, and the chest and the abdomen were emptied of the fluid, which they contained, and the liver was reduced in size, so that it could no longer be felt. This was the work of three



weeks ; and thus he was set free from the more formidable consequences of his disease. But the disease itself remained within its original seat, the heart. And in three weeks more he was dead. The heart by the simple vehemence of its action had (as it seemed) the power to kill him. Air passed uninterruptedly through every part of his lungs, yet the dyspnœa he suffered was constant, and it became an agony on the least movement. His head was racked with continual pain. He was almost without sleep. He became delirious, and then maniacal, and then convulsed, and at last sank from exhaustion of his nervous system.

The ease, however, after all, must be left incomplete : permission was not granted to examine the body after death, and thus the best proof was wanting of what the disease of the heart really was ; yet, even with this defect, the ease deserves to be recorded. For it can hardly be doubted that the disease was acute endocarditis, and it is most probable that pericarditis was conjoined with it from the first. The period of its origin was well ascertained. It sprang from one of the common causes of inflammation, and it was unconnected with rheumatism.

Dr. West, from his large experience of the diseases of children, has contributed some cases of idiopathic endocarditis. One of them is quite to our present purpose, and I shall quote it.

“ Daniel Bain, aged 11 years, living at No. 37. Thomas Street, Stamford Street, is one of 12 children of healthy parents. Nine children are still living, one died while teething, one of scarlatina, and one of pneumonia. There does not appear to be any phthisical taint in the family.

“ Daniel has had good health, with the exception of mild attacks of measles, whooping-cough, and scarlet fever; and was as well as usual until May 8. 1843, when he complained of feeling cold, and began to cough. The chilliness was succeeded by fever, and he continued gradually getting worse till the 13th, when I visited him for the first time. He had had no other medicine than a purgative powder.

“ *May 13.* — I found him lying in bed; face dusky, rather anxious; eyes heavy; respiration slightly accelerated; frequent short cough without expectoration; skin burning hot; pulse frequent and hard. The child makes no complaint, except of slight uneasiness about the left breast.

“ There is slight tenderness on pressure over the heart, with very extended dulness. The heart's impulse is not increased. A very loud and prolonged rasping sound is heard in the place of the first sound; it is loudest a little below the nipple, though very audible over the whole left side of the chest, and also distinguishable, though less clearly, for a considerable distance to the right of the sternum. Second sound heard clearly just

over the aortic valves, not distinct elsewhere, being obscured by the loudness of the bruit.

“Respiration good in both lungs.

“I ordered the child to be cupped to  $\mathfrak{z}$ vj. between the left scapula and the spine; and gave gr. j. of calomel, with the same quantity of Dover’s powder, every four hours.

“*May* 14. — Sense of discomfort at the chest relieved by the cupping. He slept well during the night, and to-day looks less anxious, though his eyes are still heavy and suffused; the skin is less hot and less dusky; pulse 114, thrilling, but not full; tongue moister than yesterday, red in the centre, coated with yellow fur at the edges; has had one copious watery evacuation; slight prominence of the cardiac region. The heart’s sounds are obscurer and more distant than yesterday; the bruit of yesterday is now manifestly a friction sound, which is louder at the base than at the apex of the heart; the first sound is altogether obscured by it, and the second is heard only over the aortic valves.

“The child has had four powders. To continue taking them every ~~six~~ hours,  $\mathfrak{z}$ j. of strong mercurial ointment to be rubbed into the thighs every six hours. Six leeches to be applied over the heart.

“*May* 15.—There was considerable difficulty in stopping the bleeding from the leech-bites, which was so profuse as to make him rather faint. He

slept tolerably during the night, and until 6 A. M., when he became light-headed, and continued so until 9 o'clock this morning, but has since lain quiet, though troubled by a dry cough.

“His appearance is much as yesterday; skin dry and hot; pulse 120, possessing the same character as before, but with less power; tongue coated at the edges, with a dry red streak in the centre; bowels open twice, motions green and watery.

“Auscultation yields the same results as yesterday. Same treatment continued, with the addition of a saline draught containing small doses of the liquor antimonialis every four hours.

“*May 16.*—General condition much as yesterday, but on the whole seems slightly improved; pulse 120, softer.

“The friction sound is no longer audible, but a loud rasping sound is heard in the place of the first sound. The second sound can now be distinguished at the apex of the heart as well as over the aortic valves, and is quite natural.

“On the 17th, the gums were slightly affected by mercury, and the bruit was thought to be softer and rather less loud. The dose of calomel was now reduced to gr. ss. every four hours, and the child was allowed a little broth.

“On the 22d his mouth was very sore, and all active treatment was discontinued on that day. The child gradually regained his strength, but the bruit accompanying the first sound continued, and was heard a month afterwards with no other



change than being rather softer and more prolonged.”\*

In how many prominent circumstances does this instance of idiopathic endocarditis run parallel to the course of endocarditis in numerous instances connected with acute rheumatism! Its accession is quickly followed by the accession of pericarditis, then both proceed together for a few days; then the exocardial murmur which belongs to the latter ceases, marking the permanent adhesion of the pericardium. But the endocardial murmur which belongs to the former remains and continues alone, and still abides, after the boy has recovered his health and strength, marking a permanent injury of the valves.

While, therefore, at present our chief knowledge of endocarditis is as an accompaniment of rheumatism, yet, whenever we catch a glimpse of it under other conditions, we recognise it as in itself pathologically the same, affecting the same course, admitting the same complications, needing the same remedies, and leading to the same events.

The following could surely be no other than a case of endocarditis. What neither its history nor its symptoms could clearly show, was sufficiently denoted by the nature of the treatment which affected its cure.

H. B., æt. 43, presented himself to me one

\* Med. Gaz. vol. xxxii. p. 738. An important paper.



morning, with a countenance full and florid, and complained of a pain and sense of weight and tightness between mamma and mamma, occupying a space large enough to require the whole hand to cover it. The pain, he said, was not constant, but was more *on* than off. It had thus endured between two and three months, and was certainly upon the increase. Lately some dyspnœa had been added to it. The dyspnœa, but not the pain, was augmented by exertion. His pulse was 80, and regular, without any peculiar character of hardness or fulness.

Auscultation found a perfectly healthy respiration; but a very loud systolic endocardial murmur at the apex of the heart. On this occasion, he, being very hot and perspiring, was examined through his shirt and flannel waistcoat. The man was a collector of taxes. He had never known illness before. He was habitually a full liver, eating abundance of animal food, and drinking plenty of beer and porter.

In this case I had no other thought than that the murmur proceeded from some tardy conversion of a portion of the mitral valve into cartilage or bone, which by its gradual increase had now reached a point at which the heart and the circulation must begin to feel it, and must ever continue to feel it, painfully and injuriously. I could not, however, tell how long the murmur had existed, and so, for the sake of creating a

little hope for myself in the management of the case, I was willing to believe that it might not have existed earlier than the date the patient gave of his own uneasiness, and that it might then have arisen from, and might still be due to, some active process of disease within the reach of a remedy; and I set about its treatment accordingly.

I ordered ten leeches to the præcordial region, and some active aperient medicine; and I enjoined a rigid abstinence from all wines and fermented drinks, and perfect rest at home. Three days afterwards, he spoke of a general sense of relief; but said the pain was more abated than the dyspnœa. I now made a more accurate auscultation of the bare chest, and found the murmur less loud. It accompanied the systole, and the limit within which it was heard included about an inch and a half of the præcordial region, viz. the mamma, and a little space on the sternal side of it and below it. There was dulness to percussion at the apex, and for a couple of inches lower down. There was no perceptible increase of impulse. The murmur, which was manifestly less while he remained still, became as loud as ever after he had walked twice across the room.

I ordered ten more leeches to the præcordial region, and two grains of calomel every six hours.

In three days more, the murmur had certainly

still decreased; there was no salivation; he was ordered to continue the calomel.

In seven days more, he was fairly salivated, and had been so five days; I was not certain of any murmur; the sounds of the heart were loudly intonated, but it was doubtful whether they were really unnatural. I wished the salivation to be still maintained by two grains of calomel taken every night.

In seven days more, upon a very patient auscultation I satisfied myself that there was still just that degree of prolongation, and that slight roughness of the first sound which one hears before the murmur is decidedly audible in acute rheumatism. Exertion brought out the real murmur, but it was very faint; no dulness to percussion remained in the situation first indicated.

Salivation to be maintained.

In seven days more, I found that since I last saw him he had been following the same plan of treatment, except that he had not observed perfect rest; he had walked about as usual in his business, but had been careful to avoid all hurry; he spoke of a *sensation about his heart*; it was not pain; he could not tell what it was, or whether it was without or within; it was something which did not naturally belong to him; was not always present, nor was it increased by exertion; but there was something which he could only call a *sensation*. After very attentively

examining him, and making him walk rapidly about my room, my ear could detect no murmur, or any other unnatural sound accompanying the movements of the heart.

If I have rightly interpreted the nature of this case, here was endocarditis arising and existing as the whole and sole disease, unaccompanied by rheumatism or by any other known malady elsewhere in the body. At all events, here was a formidable group of symptoms, all referable to the heart, and all gradually disappearing, and the murmur among the rest, under the use of remedies addressed to the purpose of arresting inflammation.

This case teaches a practical lesson of some value. It is this — that we should be slow in reckoning diseases to be incurable. The murmur was not known to have its origin in inflammation of the endocardium, but was found among other symptoms referable to the heart, which had existed between two and three months. Yet both it and they were successfully treated, and ceased. This instance, therefore, gives us encouragement to hope sometimes beyond what we know, and to direct our treatment accordingly. Where palpitation, dyspnoea, and præordial pain, of no very remote duration, are found in alliance with the endocardial murmur, we should make the most of it, as an indication of treatment, in the hope that the whole disease may depend upon a covert



and slowly progressive, but remediable endocarditis.

There is no part of pathology which calls for the more earnest regard of medical men than the diseases of the endocardium, especially with a view of making out what they are in their first formation, and noting, with clear marks of distinction, those which are simply inflammatory, and come within the possibility and promise of cure.

Of how many cases of complex disorganisation of the heart, already gone far beyond the possibility of reparation, does valvular disease form a part! In the vast majority of these, there is reason to believe that the valvular disease was the original and elementary change of structure to which all the rest was superadded as a natural and necessary consequence, and that there was a time when it existed singly and alone, without either the dilatation of the cavities, or the hypertrophy of the muscular substance, or whatever else may make up the complex disease which is at length found.

Well, then, it is this valvular disease that we want to know more about. In many instances it can be traced back to an attack of acute rheumatism, when the endocardium was inflamed. But in many more acute rheumatism forms no part of the history. What, in such instances, is the nature of this valvular disease, or rather what *was* it from the first? It is too discouraging a view



of the matter to regard all valvular disease, which is not traceable to an attack of acute rheumatism, as essentially chronic and irremediable from the beginning, and so give up further inquiry in despair.

Remember, it is not long since this district of pathology to which I am pointing, was utterly dark. It is but lately that clinical research has thrown light enough upon it to show that there is such a thing as inflammation of the endocardium; acute, rapid, lymph-depositing inflammation; inflammation in its nature curable; and, when perfectly cured, allowing the membrane to recover its integrity as if it had been never diseased; and when imperfectly cured, spoiling a valve, and leaving it to become a point of departure, from which the heart may proceed to any mode or extent of disorganisation which it is capable of. But (I repeat) it is in alliance with rheumatism that all, or nearly all, which we thus know of endocarditis has been learnt and authenticated. A glimpse, indeed, has been caught of it apart from rheumatism, enough to show that it may exist, but not enough to familiarise us with it under other conditions; not enough to bring those conditions distinctly into view, to enable us to say what they are, and to know from them when to expect it, and be prepared to treat it.

It would be well for those who feel strongly the desirableness of more knowledge upon this

subject, to consider how it is most likely to be obtained. Now I see little to hope from any more curious scrutiny of auscultatory signs, or from any further poring and pondering over symptoms immediately referable to the heart itself (the heart itself already speaks plainly enough about its diseases, if that were all); but I look with better promise to clinical research among fevers and febrile ailments of the constitution at large. For it was by the light first let in from thence, that the discovery was made within our own time of some of the most important diseases to which particular organs are liable. These diseases had for ages remained occult; and they were so because, during the period of their greatest activity, they are wont to put forth no symptoms calculated to arrest attention. But now that the diseases themselves are known, their symptoms are found to be sufficiently definite. They are, however, such as require to be sought after before they are found. But whence do we get our hint to search after them? Even from the more general and more apparent conditions of disease with which experience has found them naturally associated. Thus having typhus fever to deal with, we seek for follicular ulceration of the intestines, and often find it. Having scarlet fever, we seek for inflammation of the kidneys, and often find it. Having acute rheumatism, we seek for endocarditis, and often find it. It was, indeed,

a great thing—a thing to be valued at the worth of many lives—to find out these natural alliances of ulceration of the intestines, inflammation of the kidneys, and endocarditis, with those several more prominent and obvious conditions of disease in the constitution at large.

But, doubtless, each of these several diseases of particular organs occurs unallied with any such fevers of well-known type and character. How often, I cannot tell; probably much oftener than any one suspects. As to endocarditis, we have lit upon it a few times by accident, obscured by circumstances, but at an early and curable stage, when it would probably have been cured, had it been more clearly seen, and had it presented a steadier mark for the aim and direction of medical treatment. And, times without number, we have met with chronic valvular disorganisations which might have, and probably had, their origin in some attack of endocarditis, which was never known, and never treated.

But sometimes to light upon it by accident, and oftentimes to be hopelessly admonished by its fatal consequences that it has existed, would seem to imply that clinical observation has yet much to search after, and (it is hoped) has yet much to find, respecting the origin and progress, and various pathological relations of endocarditis.

Of what endless and still increasing necessity is this business of clinical observation to the im-

provement of our art!! All that has been found out by those who have gone before us does not leave less, but more to be sought after by ourselves. Each new fact is a mere curiosity, while its value and its uses are yet undetermined. Labour or chance may have first disclosed it, but its value and its uses can only be ascertained by long observation and experience. For, until it be seen, how other facts, already known, naturally group themselves around it, we can understand neither its place nor its bearing in the system of things to which it belongs. In this way does each newly discovered fact suggest and multiply new inquiry; and thus there are never wanting to our profession fit objects for the best understandings to pursue.

Clinical observation, with a view of keeping a man up to what is known, and perfecting him in its accustomed uses, may be an affair of sober industry only, of patient and almost passive looking on. But clinical observation, with a view of knowing more than is known, and turning new knowledge to its uses, belongs to an industry of another kind, to an energy ever active and stirring, and drawing upon, and working with, the highest faculties of the mind.

## LECTURE XVII.

PERICARDITIS INDEPENDENT OF RHEUMATISM.—SHOWN BY MORBID ANATOMY TO BE OF COMMON OCCURRENCE.—ITS SMALLER DEGREES THE MOST FREQUENT.—PROBABLY HARMLESS.—GENERALLY BEYOND THE REACH OF CLINICAL DIAGNOSIS.—ITS GREATER DEGREES NOT BEYOND ITS REACH, BUT APT TO ELUDE IT.—WHY.—*COVERT* ACUTE PERICARDITIS AND *COVERT* ACUTE PLEURISY COMPARED.—REVIEW OF CASES WITH THE PURPOSE OF FINDING WHAT NATURAL ALLIANCE PERICARDITIS MAY HAVE WITH OTHER GENERAL PATHOLOGICAL CONDITIONS BESIDES RHEUMATISM.

THERE is no structure of the body more liable to inflammation than the pericardium. Of those who have reached adult age and upwards, one half (it appears) have suffered pericarditis at some period of their existence. But then, in the vast majority of cases, it is neither detected, nor perhaps detectible, during life. It comes and goes unnoticed, and neither by itself while it remains, nor by its effects when it has ceased, does it do any amount of injury capable of interfering with the healthy actions of the heart. Hence in five cases out of six there is no clinical history to be given of pericarditis. How and when, and under what circumstances it takes place in the



living man, we have not the smallest experience. All our knowledge of it is from its effects which we discover in the corpse.

All those white spots upon the surface of the heart, which have engaged and perplexed the speculation of pathologists ever since they have betaken themselves to dissection, have at length been demonstrably shown by Mr. Paget to be the effects of inflammation. To show the absolute frequency of pericarditis, and the comparative frequency of the slighter and severer cases, Mr. Paget gives the following summary of his dissections:—“ Including these white spots among the effects of pericarditis, I find that, of 110 cases which I have lately examined at St. Bartholomew’s Hospital, 58 have presented signs of having suffered at some time from that disease. Among these, 40 out of 66 males, and 18 out of 44 females were thus affected; and, with respect to their ages, the morbid appearances were found in 5 out of 14 below twenty; in 25 out of 53 between the ages of twenty and forty; and in 28 out of 43 above forty. Of these 58 cases of pericarditis, 49 were slight cases marked by white spots and adhesions, or by effusions of small quantities of lymph; and nine were severe, with complete adhesion, or with abundant recent effusion.”\*

\* Med. Chir. Trans. vol. xxiii. p. 29.

But in the slighter cases of pericarditis, though neither pain nor any disturbed action has ever led to a surmise of its existence, yet is it not probable that the inflammation, while it was in the act of depositing the lymph which formed the white spots and the small adhesions, gave occasion to the genuine to and fro sound? Indeed, it is most probable. But what, though the sound were there, if it was not heard? And heard it could not be, if it was never listened for. And, unless attention were expressly drawn to the heart by its disturbed action, or the patient's reference of pain to it, no one would think of listening for it.

Surely, then, the reasons are plain enough why the lesser and most frequent cases of pericarditis have hitherto altogether eluded observation during life. And, until they are brought within the reach of our clinical knowledge, they cannot become the objects of our medical treatment.

Doubtless it would be to our credit, that pericarditis in all its slighter degrees should come within our knowledge and treatment. But, because this is not the case, mankind has suffered nothing. For such pericarditis is harmless from beginning to end. It puts life to no present peril, and does no ultimate injury by its effects. These white spots and slender adhesions of the pericardium are often found where there is not a vestige of *disease* besides; and then the heart at the same time is so constantly found perfect in

size, and form, and capacity, that they may be considered as things almost purely innocent.

What I am now saying of the pericardium and its covert inflammation ending in white spots and small adhesions, will call to mind what, in a former lecture, I said of the endocardium and its covert inflammation, which ended in puckering, thickening, and shortening of the valves.

But the inflammation of the two structures, while they are both alike in the secrecy with which they carry on their elementary process, are most unlike ever afterwards. While small patches of lymph and small adhesions of the pericardium are never felt injuriously—indeed, are not felt at all—a spoiled valve (and how little does it take to spoil it!) at once begins to baffle the functions, and ends by disorganising the entire structure of the heart. To the clinical study of the two diseases we are urged by very unequal motives, inasmuch as with respect to the one mankind suffers not at all, but with respect to the other it suffers to a great amount, from the darkness in which both are often involved in their beginning and progress during life.

I do not see how it is possible, but that pericarditis and endocarditis in their slighter degrees must ever continue to escape our notice, until they are found to have some constant or very frequent alliance with other forms of disease beyond the heart, which are better known and

more familiar to our experience than they are themselves.

It is probable that the attack of inflammation, leaving such permanent effects upon the pericardium as have been described, often takes place during some fevers. Doubtless during the progress of fevers, inflammation of small degree and small extent is apt to arise and to continue for a while, and then to cease without any special remedy, or any treatment more than is included in the general management of the fever. I recollect in a case of typhus fever, about the middle of its course, a loud friction-sound being heard in the præcordial region. It continued distinctly audible for three days, and then ceased altogether.—(M. xxxi. 85.)

But the very numerous cases in question which have but lately been shown to be of the nature of inflammation, or even to partake of the character of disease at all, these slighter cases of pericarditis, stand quite alone, and offer very little promise of being better understood clinically than they are at present. We will leave them, therefore, and turn to others of greater interest.

Pericarditis, it is well known, may occur in its acutest form, and yet be unconnected with acute rheumatism. Our clinical acquaintance with it as such is indeed less perfect; and on that very account, perhaps, it is the more dangerous. For our clinical acquaintance with it will appear to be



less perfect chiefly in this respect, that we have no exact knowledge of the circumstances conducing to it. And this is quite enough to make us less alert in detecting it, and less ready with our remedies in treating it.

Associated with this disease, there are certain recollections of things which made a great impression upon my mind when I first betook myself to the study of physie, and which I now feel it interesting and profitable to recal.

It was then a sort of transition period in medicine. Men were pushing their research in a new direction, but had not yet arrived at many sure results. They were inquisitive especially into what might remain of the vestiges of disease after death, as offering a promise of larger and more accurate knowledge.

In this new zeal for dissection, a vast deal of rough morbid anatomy was practised, teaching many new facts which led to some truth and to much error, the natural fruit of over-hasty conclusions.

Now, it was a long time before I could appreciate in the least degree the more important uses of the facts which dissection was daily disclosing to me. But there was one thing which I was not slow in finding out from morbid anatomy, viz. the great imperfection of the diagnostic part of medicine. That many forms of chronic disease should be often found after death, which had not



been suspected to exist during life, was no surprise to me. This is what might have been expected. But I was indeed astonished that it could ever happen (as happen it frequently did), that patients who had been carefully watched day by day, should, when they came to die, be found to have perished of acute destructive inflammation of some vital organ, which had never been suspected to exist while they were alive.

It was the chest which was most frequently the seat of this acute, covert, and mortal disease; and of the parts within the chest, most frequently the pleura or the pericardium. To my amazement, the pleura or the pericardium were occasionally found covered with recent lymph, and their cavity full of turbid purulent fluid, where during life there had been neither thought nor mention nor treatment of pleurisy or pericarditis.

The students were the chief morbid anatomists in those days, who yet knew nothing of the difficulties of their profession, and made no allowance for them. To such young, sharp-sighted, inexperienced lookers-on, these unhappy instances would minister occasion for ridicule of their betters. They would pass harsh judgment on the physician, and express opinions not very complimentary to his sagacity.

But was the fault in the physician, or in the imperfection of his art? I trust it was in the imperfection of his art; if it were not so, I ought

to have some painful reflections. For I confess (and it is my duty to confess) that the experience of after years and the best care and watchfulness I could bestow upon individual cases, did not exempt me, in my turn, from the occasional mortification of finding upon dissection that a patient had died of an acute pleurisy, or an acute pericarditis, which I had never suspected during life; of pleurisy, however, much oftener than pericarditis. For, as an acute, disorganising, destructive inflammation, pleurisy is by far the more frequent of the two.

Such painful oversights are now not apt to occur; not that physicians in general, or the same individual physicians, were less sagacious formerly than they are now. There is no room for disparaging our predecessors and glorifying ourselves, or for complimenting our present selves at the expense of our former selves, in this matter. The truth is, a discovery has been made in the art of clinical observation, and we all have the benefit of it. Without the aid of auscultation, it is impossible but that the same diseases should still often go undetected. A few cases of pleurisy present themselves to me in the course of every twelvemonth, which, but for the use of my ear, I should detect either not at all, or not time enough to interfere effectually for their relief.

But before auscultation was practised, pericarditis (I mean acute, disorganising, destructive

pericarditis) did not go so often unnoticed and untreated as acute pleurisy. Yet there was the same want of exact pathognomonic signs to designate the one as the other. But then, with respect to pericarditis, this most important fact had already been learnt by experience, that, though its own direct signs were vague and uncertain, it was apt to be associated with other conditions of disease as plain and obvious and distinctive as possible, those, namely, which constitute acute rheumatism. Thus, wherever there was acute rheumatism, we knew that pericarditis might be; and we were sedulously on the watch for it, and ready to take the least præcordial flutter or pain as an evidence of it, and a warrant for treating it. We made sure of it from a mere glimpse, and pointed our remedies at it as if it was clearly in our view.

But when it occurred alone, or among other conditions of disease, with which it was not known to have any frequent or natural alliance, it ran a great chance of escaping detection and treatment; and it often *did* escape both, and proved rapidly fatal.

And even now, with all the sure signs which auscultation has unlocked and brought from their hiding-place, it still is apt to escape detection and treatment, and consequently to prove fatal. Nearly the whole sum of my experience of acute pericarditis, independent of rheumatism, is de-

rived from cases in which it has been concealed during life, some occurring prior and some subsequently to the use of auscultation. In the former it could not possibly have been detected for want of auscultation; and in the latter it lay concealed, because auscultation went for nothing when the auscultatory signs were not listened for.

I will now produce some specimens of pericarditis, as I have seen it occur independent of acute rheumatism. The sort of patients in whom it is found, and its attendant circumstances, may, perhaps, give some hint of the pathological conditions conducing to it.

W. B., ætat. 26, a pale, bloodless, emaciated being, was carried into St. Bartholomew's Hospital, retching and vomiting, and coughing incessantly; he had a pale, dry, rough tongue, without the least secretion upon it; his pulse was hardly perceptible, and all things bespoke him to be at the point of death. He had been brought to his present condition by an illness of five weeks. Five weeks ago he had been attacked with purging of blood, and with vomiting or spitting of blood (he could not tell which), at the same time; and these hæmorrhages continued, in some degree, for an entire fortnight.

He was by trade a journeyman tailor, a class of society which has furnished me with more cases of profuse hæmorrhage from the stomach



and bowels than any other, and in which the habit of spirit-drinking is carried to the most horrible extent.

After his admission the retching and vomiting continued, but there was no hæmorrhage, and the evacuations from his bowels were natural; he became delirious; and in a day or two, delirium and stupor became his most prominent symptoms; and what little pulse could be perceived, was irregular. It was necessary to give him wine; and sometimes, under its influence, the pulse would gain a little power, and recover its regularity, but it would soon lose its power, and then it would become irregular again. A week passed, and he continued to live on, when erysipelas appeared upon his face and neck, and in three days more he died.

In this case there was no symptom which could possibly suggest a suspicion of disease of the heart, except the irregular pulse. Yet it suggested (I confess) no such suspicion to me; I saw in it only the last struggling effort of the heart to keep up life a little longer. When the pulse became *stronger* under the stimulus of wine, it for the time became *regular* also; and when, as the stimulus ceased, it again lost its power, it again became irregular. All this looked very like failure from simple weakness.

But, upon dissection, the pericardium was found to contain several ounces of serum, deeply tinged



with red, and covered both on its loose and reflected portions with a very large accumulation of lymph. The lymph connected the two opposite surfaces by filamentous bands of soft texture, and was easily detached. Besides these unlooked-for appearances of the pericardium, the mucous membrane of the stomach was stippled with points of red, and was softer than natural. Throughout the whole intestinal canal, there was a redness, which seemed to be something different from simple injection of the bloodvessels, and to reside in the submucous cellular tissue. The peritoneum was pale, and contained within its cavity a considerable quantity of clear serum. The brain and its membranes were exsanguine, while there was much serum beneath the arachnoid. The lungs were sound.—(M. iii. 74.)

Here we had disclosed by dissection the sure effects of acute disorganising inflammation of the pericardium; and this it was that killed the patient. Yet were there no general symptoms notifying such inflammation any where, and no pain or other sign immediately referable to the heart, notifying that it could be there, except the almost imperceptible and irregular pulse; and this seemed to speak of *death* rather than of disease.

But this case fell under my observation before any thing was yet known of the auscultatory signs which mark disease of the pericardium.

Yet had they then been as well understood as they are now, I was so entirely without any suspicion of the heart, that I doubt whether I should have applied my ear to it.

Now I have dwelt more particularly on this case, because it is one of a class (such, at least, is my impression) to which a peculiar pathological interest belongs. And it is a good specimen of that class.

It would hardly be suspected that the very act and process of dissolution could give occasion to *new* disease. But such is the fact. And it happens especially, if the dissolution be slow and lingering; and then this new disease is often even of an acute kind. In no part of the body is this new disease more apt thus to light up, at the very going out of life, than in serous membranes. Among phthisical patients, who have been dying by little and little for many weeks, the instances have been numerous, in which upon dissection I have found the marks of very recent peritonitis, the cavity of the abdomen containing a whey-like fluid, and the surface of the intestines covered with flocculent lymph, and streaked with red, and adherent where their folds lie in contact. Yet in many such cases the peritonitis has given no notice of its existence during life by its proper symptoms, and after death has occasioned great surprise by its discovery. And thus, too, pericarditis will arise when the system is at its lowest

state of depression. I have known some instances (and others have been reported to me), where, after severe accidents and severe surgical operations, the powers of life being brought very low, and existence with difficulty maintained during some days, upon death eventually taking place, the pericardium has been found covered with flocculent lymph, and its cavity distended with serum mixed with pus and blood. These were the products of the most acute inflammation. But the patients were scrupulously watched during life, yet no symptoms indicative of inflammation were discovered.

A young man of unfavourable constitution suffered inflammation of the internal structure of both eyes. He was largely bled, and brought rapidly under the influence of mercury. Great dyspnœa arose with the salivation. Both of his lungs and his larynx became inflamed. He passed into a state of coma, and after lingering for a few days he died. Upon dissection, besides effusion between the membranes and into the ventricles of the brain, and ulceration of the larynx, and hepatisation of the lungs, and a pint of fluid in the left pleura, there was found upon the surface of the pericardium covering the heart a large deposition of soft recent lymph, particularly about the origin of the great bloodvessels, and the muscular substance of the heart itself pervaded by a white interstitial deposit (apparently lymph)

which was thickest near the pericardial surface. — (M. xx. 56.)

A few instances have occurred within my knowledge of individuals having been picked up in the street, and brought into the hospital in a dying state, who, nevertheless, have survived for a few days, and afforded time to investigate the conditions of their disease. Nothing, however, was made out concerning them, but that they were dying; and not the least conjecture could be formed where their disease was, or what it was. Upon dissection, the pericardium was found covered with lymph, and its cavity distended by turbid serum.

I think it worth while to add the following cases, as further specimens of the conditions under which pericarditis is apt to arise, and of its complications. They are cases which contain many points of great interest.

W. C. was a poor boy, ten years of age, who came into the hospital to die after an illness which he had already endured eight months. His whole body was œdematous, and his abdomen full of fluid. He was very pale; he was too weak to stand; and his pulse was rapid, small, and feeble.

The account given of him was this:—eight months previously, he was seized with vomiting of green matter, and three days afterwards with purging of blood. This latter continued for several days, and, upon the whole, the quantity of

blood lost was very large. The hæmorrhage having ceased never returned; but a few days afterwards, œdema commenced in the lower extremities, and in the course of a fortnight pervaded the whole body; fluid was then accumulated in the cavity of the abdomen. All medicines failing to give relief, at the expiration of two months he was tapped. Hereupon his kidneys began to act profusely, and in six weeks he was entirely free from all dropsical swelling, and he continued free until within five weeks of his admission into the hospital; then the dropsical symptoms no sooner returned, than they increased rapidly, and, in the course of a fortnight, they reached their extreme amount.

The poor boy seemed dying when he was brought in, yet he lived a fortnight. His days and nights were passed in extreme jaetitation and perplexity, in delirium, and vomiting, and struggling for breath. His urine was loaded with albumen, and auscultation detected fluid in the chest. Three days before his death, a loud creaking crumpling sound was heard to accompany the contractions of the heart. It was audible over a large part of the chest in front. (The heart had been carefully examined before, and its sounds were noted to be "not unnatural.") The crumpling sound was accompanied by some increase of impulse; but in twenty-four hours it ceased to be audible.



Dissection found the internal collections of fluid, and the granulated kidneys, and the pale softened textures, which were anticipated; and it found withal on the surfaces of the pericardium covering, and opposite to both the auricles, a network of lymph, in which numerous soft granules were deposited. The perieardium was unusually vascular throughout, but especially at the parts from which the lymph was detached. The muscular substance of the heart, and its internal lining, and that of the aorta, were unusually red. —(M. xvii. 29.)

Here to accumulated suffering and disease, which probably first sprang from inflammation of the kidneys, pericarditis was at length added, and rapidly brought on death.

In like manner, pericarditis came and closed the scene in the next case. One opportunity only was afforded for examination during life, but it was enough to give suspicion that disease was at work upon the heart, wherever else it might be.

W. G. ætat. 50, was admitted by mistake into the surgeon's ward, whence he was presently transferred to me. He was nearly comatose, and naturally almost deaf. He lay on his back with his muscles quivering, and muttering occasionally to himself. His left eye was inflamed superficially, his jugular veins distended, but without pulsation. His pulse was 72 with power, and he flinched from pressure on the epigastric region. We suc-

ceeded in rousing him a little, but could not succeed in getting any rational expression from him, except to the effect that he felt no pain.

We could only auscultate his chest as he lay; that is, in front; and found a rhonchus mixed with large crepitation proceeding generally from the lungs, and a very peculiar bruit from the heart, which accompanied the systole of the ventricles.

Hereupon he was cupped upon the præcordial region.

In twenty-four hours he was dead.

Here, too, upon dissection, the kidneys, large, hard, pale, and mottled, and entirely disorganised by granular disease, presented themselves as the source of morbid actions, which had been going on throughout the whole body for years. On the left side, the lungs floated in three pints of bloody serum, and on the right, in two, themselves being gorged with blood. In the cavity of the peritoneum, two pints of yellow serum were effused without apparent disease of any abdominal viscus. The pericardium contained three ounces of serum; and the membrane itself, both its loose and reflected portion, was covered with a fine, reticulated, adventitious membrane of coagulable lymph, most abundant over the left cavities. It was easily detached, and left the surface beneath studded with red spots. The heart was rather

large, but well proportioned in all its parts.—(M. xiii. 39.)

Such are the specimens I have to give of pericarditis, not in alliance with rheumatism. In them all, you will observe it complicated with disease of other internal parts, and especially with disease, or the results of disease, of similar structures; with inflammation, or serous and sanguineous effusion, of the pleura or peritoneum. In short, of pericarditis not in alliance with acute rheumatism, I do not know that a single uncomplicated case ever fell under my observation. I have, therefore, been the more curious to look into the experience of others, and see what they have found.

Of pericarditis, not in alliance with rheumatism, Corvisart gives five cases; and it was complicated with inflammation of other parts in all of them except one; and in this one it was caused by a severe blow upon the region of the heart. Two of his cases ran closely parallel with those which I have related, as in other respects so especially in this, that the pericarditis arose at last, and brought a long previous illness to a fatal termination.

A young Creole lady came from Martinique to Paris. When she was confined, and for six months afterwards, she managed herself ill in the affair of nursing. Corvisart was called to visit her when she was actually dying, and could only learn that, for six days, she had been suffering

obscure and indefinite symptoms, and had pointed to the heart as the seat of her pain. Her pulse was irregular, and her jactitation extreme.

On dissection, pleuro-pneumonia, especially of the right side, was found; and pericarditis, which had filled the cavity with sero-purulent fluid, and spread a thick coat of lymph over the entire surface of the membrane. Here was no diagnosis of pericarditis during life.\*—(Obs. ii.)

An old Creole naval officer, the victim of gout and venereal indulgences, came from Martinique to Paris. Here, after being fatigued, he was attacked with low fever. In the course of the fever he became delirious, and suffered an incomplete paralysis of the left arm. Then his breathing was impeded, then his pulse was irregular, and he died.

On dissection, serum was found abundantly effused between the membranes of the brain and into the right lateral ventricle, the inferior lobe of the right lung hepatised, and the right pleura covered with lymph and a sero-purulent fluid in its cavity, and the pericardium containing the same kind of fluid, with a plaster-like substance upon its surface. Here, too, there was no diagnosis of pericarditis during life.—(Obs. iii.)

Another case was unlike these, and unlike any which I have given. A strong middle-aged

\* Corvisart, *Essai sur les Mal. du Cœur.*

man was seized, in the midst of health and without any obvious cause, with dyspnœa, and an acute pain in the lower and left region of the chest, and on the same night with a tremendous shivering fit. On the day but one afterwards he was admitted into the hospital, already subdued by his disease, with his pulse small and fluttering and irregular, his features collapsed, convulsive movements of the face, hurried respiration, and pain in the præcordial region. It did not appear that any treatment had hitherto been employed, and what was now practised was useless. In two days more he died.

Here were found, upon dissection, the effects of pleuro-pneumonia of the left lung, which involved the diaphragm, and of extensive pericarditis. But there was evidently no distinct anticipation of pericarditis from the symptoms. —(Obs. i.)

In another case the disease, whatever it was, was successfully treated; but that it was pericarditis, one may be allowed to doubt. Pleurisy was a part of it, if not the whole. The affection came on, after great exertion, suddenly, with a violent rigor, and with extremely severe pain—first in the region of the heart, and soon extending itself over the entire left side of the chest with dyspnœa, delirium, and insupportable anxiety. The patient was a female, and the catamenia occurring on the third day of the disease seemed to carry away its most alarming



symptoms, although no remedy whatever had hitherto been employed; the catamenia stopped, and the symptoms returned with their first intensity. On the 10th day from her original seizure, she was admitted into the hospital, with symptoms which would, *now*, be considered to denote the left cavity of the pleura full of fluid, and its inflammation relieved, but not entirely cured, by the effusion. After remaining twenty-three days under judicious antiphlogistic treatment, she was discharged convalescent.—(Obs. v.)

The single uncomplicated case remains to be noticed, where pericarditis was caused by a severe blow upon the region of the heart. It might have been a fortunate specimen for studying the disease in its simplest character, had it but been seen earlier. For not only was it an inflammation of the pericardium exclusively, but its cause was of the simplest kind, viz. mechanical. Its proper symptoms, however, as a disease, were past before the patient reached the hospital, and what remained were its irremediable and inevitably fatal effects. But still there is a very great interest belonging to this case, and that interest is contained in the uncomplicated character of the disease, and its simple mechanical cause.

The patient, it seems, was dying when he was admitted. His pulse was “small, frequent, unequal, intermittent, and irregular;” his eyes were “sunk,” his features “much changed.” One

remedy was ventured upon—a bleeding; but it was not repeated, and nothing else was tried. Heneeforth, to his death, the record of his state is very succinct, and is comprised in “countenance more and more hippocratic; breathing continually interrupted, and very difficult; pulse vacillating, and scarcely perceptible; prostration of strength extreme notwithstanding the use of cordials; a spontaneous and almost sudden dissolution of the right eye, from a suppuration which took place in it without being preceded or attended by any inflammatory symptoms; at length, features entirely changed; pulse imperceptible; debility extreme, even to fainting; death.” But though dying when he was admitted, and though every recorded symptom thenceforth gave notice and threatening of death, he did not actually die until after the lapse of nineteen days; and after death the enormous quantity of nearly four pints of sero-purulent fluid was found distending the bag of the pericardium, and its whole surface covered with a thick crust of reticulated albumen. Except that the left lung was pushed upwards, while it still was spongy and erepitous, there does not seem to have been the least change, either morbid or mechanical, found in any other internal organ of the body.

What a vast amount of disease was here accumulated upon the heart! How is it possible that the heart could bear it so long, or bear it at all?

Why, consider all was sound and perfect throughout the body, from first to last, except the heart. Its disease reached it from no previous malady of the constitution. It was received from no other organ *by* it, and it was imparted to no other organ *from* it. It began, proceeded, and ended in the heart. — (Obs. iv.)

Such are Corvisart's cases, and such the sum of what it has appeared to me useful to remark upon them.

Andral gives six cases of pericarditis not in alliance with rheumatism, of which three were complicated, and three were not.

His three complicated cases have a remarkable coincidence with some of those which I have given from my own experience. In one, the pericarditis was complicated with tubercles and vomicæ of the lungs; in another, with chronic asthma and bronchial congestion; in the third, with petechial small-pox. In all three there was reason to believe, that it came on just prior to dissolution, and in none of them was it the object of clinical diagnosis during life. Andral, *Mal. du Cœur*. — (Obs. v. vi. vii.)

Of his three uncomplicated cases one, which recovered, is not regarded quite with certainty by Andral himself as a case of pericarditis. It probably was so. Its symptoms set in with fever, vertigo, and apparent cerebral congestion, which were followed by severe pain in the præcordial

region and epigastrium, and very irregular and tumultuous action of the heart. These yielded to venæsection, and several applications of numerous leeches, and the patient got well. — (Obs. iv.)

Another is a case of exceeding interest. The patient, a shoemaker, thirty-one years of age, was admitted into the “Hôpital de la Charité” on the third day after his attack, and gave this account of himself:—that, three days before, he had been seized with rigor and a general sense of illness, and that during the following night he had much fever, and the next day he felt a very sharp pain in the left breast. The next day, the day of his admission, this pain continued. The following was the record now taken of his symptoms:—Countenance pale and expressive of suffering and inquietude; a sardonic smile from time to time, and a sort of convulsive trembling of the lips. Pain in the præcordial region habitually of no great amount, but now and then becoming much more severe, and then not confined to the seat of the heart, but passing, like strokes of fire (such was the patient’s comparison), into the whole left side of the thorax; while at the same time the entire left arm was seized with numbness, which would be exchanged, during a few seconds, for a very sharp pain. Whenever the pain was thus exasperated, and darted in these directions, the breathing was at once greatly impeded, the beats of the heart became tumultuous, and indescribably irre-



gular, the pulse imperceptible, and the extremities icy cold.\* But the pain no sooner abated again, than the breathing lost its agony, and became only moderately embarrassed, the beats of the heart were again regular and forcible, and heard over the whole anterior part of the thorax, and the pulse was raised a little, but always remained very small relatively to the force of the heart's impulse. The chest was every where resonant to percussion, and the respiratory murmur was every where strong and clear.

What a strange group of symptoms we have here! Distinct paroxysms of angina pectoris following close upon fever and pains, which had newly arisen and seemed to denote acute inflammation! Let us see how it all will end. The disease was yet only four days old, when this was the condition it had brought the patient to.

The great force of the heart's impulse in the intervals of the paroxysms led to the employment of venæsection and numerous leeches, and during the following night the paroxysms of pain were less severe and less frequent. And so they were during the following day, when they returned only three or four times; but they were still of the same character and extent, and still accompanied by the same numbness and pain of the whole arm down to the hand. In the evening,

\* This sudden agony has already been noticed as incident to rheumatic pericarditis. — p. 255.



another venæsection was adopted. The next night was good; and on the next day the report is given in these few words — “almost the same state.” Nothing could well be more promising than the patient’s condition on the ensuing day, which was the sixth of his disease. All pain was gone. His aspect had become more natural. The impulse of the heart was of less force and less extent. The beat of the arteries, however, was still extremely small. But now came the change. A few hours after the physician’s visit, without any return of pain, great dyspnœa arose, which went on increasing until the next morning; when the pulsations of the heart, which had all along been so energetic and forcible, could scarcely be heard within the chest; and the beat of the arteries was smaller than ever, and the entire præcordial region and a considerable space beyond it were dull to percussion. The horizontal posture was impossible, and as the patient sat erect, he could scarcely utter a few intelligible words in a gasping voice, and express that he felt, as if a chain of iron was being drawn tight round his chest, and was suffocating him. He died in the night.

On dissection, the bag of the pericardium was found distended with blood. It contained nearly a quart of a brownish-red fluid, having the sensible qualities of blood drawn from a vein. The internal surface of the pericardium was lined with

membranous concretions stained red. No other parts of the body had undergone any change, save what belonged to sanguineous engorgements and congestions, which were evidently secondary.—(Obs. iii.)

A woman, twenty-six years of age, mother of two children, and having lately suffered a miscarriage, was admitted into La Charité in such a state of delirium as made it impossible to gain from her any information respecting her previous condition. She preserved an obstinate silence, and, being interrogated, put on a fixed look, but answered nothing. Her face was pale, and her lips were kept apart, and agitated from time to time with a convulsive trembling. Her pulse was frequent and small, and her skin had little heat. The two next days there were observed frequent tossings of the head backwards, and sudden jerkings of the trunk upwards, and twitching of the tendons. She spoke, and seemed to understand; but her discourse was full of incoherence. Her pulse was intermitting, as well as very frequent. On the following day, the fourth from her admission, the delirium ceased, and she only complained of great weakness; but the muscles of the face were almost continually agitated with convulsive movements, and the upper extremities presented from time to time a cramp like tetanus. On the fifth day the delirium returned; the features motionless and changed;

the upper extremities being raised, fell by their own weight, as if paralytic; she passed into a comatose state, and died in the evening.

When the body came to be opened, neither the brain, nor the spinal marrow, nor their investing membranes, nor the intestinal canal in its entire length, except that here and there it presented a slight injection, nor the lungs, except that they were slightly engorged at their posterior part, nor indeed any other organ, offered a trace of disease; but only the heart. Yet not the substance of the heart, nor any of the vessels coming to it and going from it, but the pericardium alone. The pericardium was lined with albuminous conerctions, from which soft bands of adhesion proceeded from one surface to the other, while there were some ounces of greenish flocculent serum in its cavity.—(Obs. viii.)

Now I can only hope that this lecture of details has not drawn too much upon your patience; for I can hardly expect that your interest has kept pace with the recital of cases which I have given from my own observation and that of others. But these cases must be allowed to contain many important facts, which, not being able to reduce them under any general head, I could not have brought forward at all, but as they were exhibited in particular instances. Be it, however, remembered, that all our knowledge was originally derived from cases. And cases must still be noted

and preserved, and studied, as records of what we know, until we arrive at more general facts or principles than we have yet reached. For general facts or principles, well ascertained, are found both to comprehend numerous particulars, and to become at the same time their representatives, and so to dispense in some measure with the necessity of detailing them.


The subjects of our profession require to be treated summarily or in detail, according to the degree of light that is brought to bear upon them from a general pathological principle. If you enter a spacious room with a small taper, you must carry it about, and pick your way with it into corners and recesses, and round pillars and projections, and after all you will hardly know where you are, and will be lucky if you escape accidents. But if you enter the same with a bright burning lamp, you have only to place it on a pedestal, and then stand in the midst and look around; and then you will find all things, great and small, near and remote, brought out equally to view, and will at once understand and admire the beauty and proportions of the whole apartment.

So it is with our clinical inquiries. We must deal much in detail, we must note cases one by one, while we yet want a great pathological principle, which can show their natural relations and reconcile them together. But once establish

such a principle, and it will compass and illustrate perhaps a hundred particulars at once, and render their minuter examination needless and superfluous.

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